

Digitized by the Internet Archive  
in 2009 with funding from  
University of Toronto

<http://www.archive.org/details/s5philosophicalm25lond>



THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

CONDUCTED BY

SIR ROBERT KANE, LL.D. F.R.S. M.R.I.A. F.C.S.

SIR WILLIAM THOMSON, KNT. LL.D. F.R.S. &c.

AND

WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

---

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. l. Not.*

---

VOL. XXV.—FIFTH SERIES.

JANUARY—JUNE 1888.

---

LONDON:

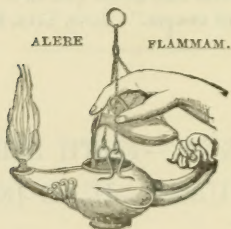
TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET.

SOLD BY LONGMANS, GREEN, AND CO.; KENT AND CO.; SIMPKIN, MARSHALL, AND CO.;  
AND WHITTAKER AND CO.;—AND BY ADAM AND CHARLES BLACK, AND  
T. AND T. CLARK, EDINBURGH; SMITH AND SON, GLASGOW;—  
HODGES, FOSTER, AND CO., DUBLIN;—PUTNAM, NEW YORK;—  
VEUVE J. BOYVEAU, PARIS;—AND ASHER AND CO., BERLIN.

“Meditationis est perscrutari occulta; contemplationis est admirari  
perspicua . . . . Admiratio generat quæstionem, quæstio investigationem  
investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,  
Cur mare turgescat, pelago cur tantus amaror,  
Cur caput obscura Phœbus ferrugine condât,  
Quid toties diros cogat flagrare cometas,  
Quid pariat nubes, veniant cur fulmina cœlo,  
Quo micet igne Iris, superos quis conciat orbes  
Tam vario motu.”

*J. B. Pinelli ad Mazonium.*



18063

13/11/91  
q.

QC

1

Pl

sér. 5

v. 25



# CONTENTS OF VOL. XXV.

(FIFTH SERIES).

NUMBER CLII.—JANUARY 1888.

	Page
Mr. E. F. J. Love on a Method of Discriminating Real from Accidental Coincidences between the Lines of different Spectra; with some Applications. (Plate I.) . . . . .	1
Rev. O. Fisher on the Mean Height of the Surface-Elevations, and other Quantitative Results of the Contraction of a Solid Globe through Cooling; regard being paid to the existence of a level of no strain, as lately announced by Mr. T. M. Reade and by Mr. C. Davison . . . . .	7
Mr. H. E. Armstrong on Valency, especially as defined by Helmholtz . . . . .	21
Mr. T. H. Blakesley on a Geometrical Determination of the Conditions of Maximum Efficiency in the case of the Transmission of Power by means of Alternating Electric Currents	30
Prof. Tait on the Motion of a Gas "in Mass" . . . . .	38
Mr. A. E. H. Love on Dedekind's Theorem concerning the Motion of a Liquid Ellipsoid under its own Attraction. . . . .	40
Mr. H. Tomlinson on the Effect produced on the Thermo-electrical Properties of Iron when under Stress or Strain by raising the Temperature to Bright-red . . . . .	45
Prof. H. Lamb on the Theory of Electric Endosmose and other Allied Phenomena, and on the Existence of a Sliding Co-efficient for a Fluid in contact with a Solid . . . . .	52
Proceedings of the Geological Society:—	
Prof. T. G. Bonney on some Results of Pressure and of Intrusive Granite in Stratified Palæozoic Rocks near Morlaix, in Brittany. . . . .	71
Prof. T. M'K. Hughes on the Position of the Obermittweida Conglomerate . . . . .	71
Prof. T. G. Bonney on the Obermittweida Conglomerate: its Composition and Alteration . . . . .	72
Prof. T. G. Bonney on the Huronian Series in the neighbourhood of Sudbury (Canada) . . . . .	72
Recent Discoveries of Gold in the Transvaal . . . . .	73

	Page
Dr. A. Geikie on the Age of the Altered Limestone of Strath, Skye .....	73
On Electricity produced by the Friction of Drops, by J. Elster and H. Geitel .....	75
A New Instrument for the Measurement of Radiation, by C. C. Hatchins .....	76
Apparatus for Illustrating the Fall of Bodies in a Vacuum, by J. Puluj .....	77
Attempt to Apply the Diffusion of Gases and Vapours through Porous Bodies to Determining the Amount of Moisture and Carbonic Acid in the surrounding Air, by F. Schidlowsky..	77
On Electrical Transportation in Liquids, by S. Tereschin ....	78
Electrical Impressions, by K. Tschechowitsch .....	80

## NUMBER CLIII.—FEBRUARY.

Prof. L. Boltzmann on some Questions in the Kinetic Theory of Gases.....	81
Mr. H. Tomlinson on the Recalescence of Iron .....	103
Sir W. Thomson on Cauchy's and Green's Doctrine of Extra-neous Force to explain dynamically Fresnel's Kinematics of Double Refraction.....	116
Mr. S. H. Burbury on the Diffusion of Gases; a Reply to Professor Tait .....	129
Mr. O. Heaviside on Electromagnetic Waves, especially in relation to the Vorticity of the Impressed Forces; and the Forced Vibrations of Electromagnetic Systems .....	130
Mr. R. M. Deeley on a Theory of Glacier Motion.....	156
On the Influence of Light upon the Electric Discharge, by E. Wiedemann and H. Ebert .....	162
Application of the Deci-ampere or Centi-ampere Balance to the Determination of the Electromotive Forces of Voltaic Cells .....	164

## NUMBER CLIV.—MARCH.

Mr. J. Buchanan on a Law of Distribution of Molecular Velocities amongst the Molecules of a Fluid .....	165
Prof. S. P. Thompson on the Price of the Factor of Safety in the Materials for Lightning-rods.....	170
Prof. P. G. Tait on some Questions in the Kinetic Theory of Gases: reply to Prof. Boltzmann .....	172
Mr. T. Gray on the Application of the Electrolysis of Copper to the Measurement of Electric Currents .....	179



	Page
Mr. F. Y. Edgeworth on a New Method of reducing Observations relating to Several Quantities .....	184
M. E. van Aubel on the Influence of Magnetism and Temperature on the Electrical Resistance of Bismuth and its Alloys with Lead and Tin .....	191
Mr. O. Heaviside's Note on a Paper on Electromagnetic Waves .....	202
Mr. T. M. Reade on the Geological Consequences of the Discovery of a Level-of-no-Strain in a Cooling Globe .....	210
Prof. J. D. Everett on the General Laws of Brightness of Images .....	216
Mr. J. F. Heyes on the Tetravalency of Oxygen; with Remarks on the Varying Valency of the Elements and the Present Chemical Aspect of the Valency Problem .....	221
Proceedings of the Geological Society:—	
Prof. J. Prestwich on the Correlation of some of the Eocene Strata in the Tertiary Basins of England, Belgium, and the North of France .....	238
Prof. J. F. Blake on the Cambrian and Associated Rocks in North-west Caernarvonshire .....	239
Mr. R. D. Oldham on the Law that governs the Action of Flowing Streams .....	240
Rev. A. Irving on the Stratigraphy of the Bagshot Beds of the London Basin .....	241
Rev. A. Irving on the Red-Rock Series of the Devon Coast Section .....	241
An Experiment on the Emission of Light by Ignited Bodies, by F. Braun .....	242
On Capillary Analysis: a Method of Separating and Recognizing Individual Colouring-matters in their Mixtures, by F. Goppelsroder .....	244
On the Convection of Electricity by Evaporation, by Dr. E. Lecher .....	244

---

#### NUMBER CLV.—APRIL.

Mr. Louis Bell on the Absolute Wave-length of Light .....	245
Hon. R. Abercromby on the Height, Length, and Velocity of Ocean Waves .....	263
Dr. T. S. Hunt on the Integral Weight of Water .....	269
Dr. H. W. Watson on the Electromotive Force in Moving Conductors .....	271
Prof. J. Young's Sketch of a Stratigraphical Table, chiefly for Western Europe .....	274
Messrs. W. W. H. Gee and H. Holden's Experiments on Electrolysis.—Part I. Change of Density of the Electrolyte at the Electrodes .....	276

	Page
Prof. S. P. Thompson on a Modified Water-dropping Influence-machine .....	283
Mr. H. Tomlinson on the Effect of Magnetization on the Thermoelectrical and other Physical Properties of Bismuth .....	285
Mr. T. H. Blakesley on a Method of Determining the Difference between the Phase of two Harmonic Currents of Electricity having the same Period .....	295
Mr. J. F. Heyes on Valency, Validity, and Residual Affinity .....	297
Mr. F. J. Smith on the Application of Hydraulic Power to Mercurial Pumps .....	313
Prof. A. Righi on some Electrical Phenomena provoked by Radiation .....	314
Proceedings of the Geological Society :—	
Prof. A. H. Green on the Geology and Physical Geography of the Cape Colony .....	316
Mr. E. Wethered on the Examination of Insoluble Residues obtained from the Carboniferous Limestone at Clifton .....	319
Mr. T. M. Reade on an Estimate of Post-Glacial Time .....	319
Mr. C. Davison on the Movement of Scree-Material ....	320
Mr. G. A. J. Cole on some Additional Occurrences of Tachylite .....	321
On the Production of Intense Magnetic Fields, by Prof. Stefan .....	322
Experiments on that Form of the Discharge of Electricity known as St. Elmo's Fire, by A. v. Obermayer .....	323
Influence of Light on the Thermal Conductivity of Crystallized Selenium, by MM. Bellati and E. Lusana .....	324

---

### NUMBER CLVL.—MAY.

Mr. T. P. Dale on the Numerical Relation between the Index of Refraction and the Wave-length within a Refractive Medium, and on the Limit of Refraction .....	325
Mr. T. M. Reade on Tidal Action as an Agent of Geological Change .....	338
Dr. A. Grünwald on Mathematical Spectral Analysis of Magnesium and Carbon .....	343
Mr. L. Bell on the Absolute Wave-length of Light.—Part II. ....	350
Mr. H. Tomlinson on the Temperature at which Nickel begins to lose suddenly its Magnetic Properties .....	372
Mr. O. Heaviside on Electromagnetic Waves, especially in relation to the Vorticity of the Impressed Forces; and the Forced Vibrations of Electromagnetic Systems .....	379
Mr. J. Parker on the Thermodynamics of Cryohydrates ....	406



	Page
Mr. C. V. Boys's Experiments with Soap-bubbles. (Plate II.)	409
Mr. R. H. M. Bosanquet on the Use of the term "Resistance" in the Description of Physical Phenomena.....	419
M. W. Michelson's Theoretical Essay on the Distribution of Energy in the Spectra of Solids .....	425
Notices respecting New Books :—	
Prof. W. C. Unwin's Testing of Materials of Construction	436
Proceedings of the Geological Society :—	
Mr. H. Fox on the Gneissic Rocks off the Lizard .....	438
Rev. J. F. Blake on the Monian System.....	439
Prof. V. Ball on some Eroded Agate Pebbles from the Scudan .....	442
Prof. V. Ball on the probable Mode of Transport of fragments of Granite and other Rocks which are found imbedded in the Carboniferous Limestone of the neighbourhood of Dublin .....	443
Messrs. Gardner and Keeping on the Upper Eocene, comprising the Barton and Upper Bagshot Formations ..	444
Mr. W. Hill on the Lower Beds of the Upper Cretaceous Series in Lincolnshire and Yorkshire .....	445
Dr. Hicks and Mr. De Rance on the Cae-Gwynn Cave, North Wales .....	446
On the Thermal Conductivity of Hard and Soft Steel, by F. Kohlrausch .....	448
Mountain Formation, by Joseph LeConte .....	450
On the Action of Light on the Electrical Conductivity of the Haloid Salts of Silver, by Svante Arrhenius .....	451
Glacial Motion, by Joseph LeConte .....	452

---

### NUMBER CLVII.—JUNE.

Mr. W. E. Sumpner on the Variation of the Coefficients of Induction. (Plate III.) .....	453
Profs. W. E. Ayrton and J. Perry on the Efficiency of Incandescent Lamps with Direct and Alternating Currents	476
Mr. E. C. Rimington on the Measurement of the Power supplied to the Primary Coil of a Transformer .....	482
Dr. C. H. Draper on the Polarization of Platinum Plates....	487
Profs. W. E. Ayrton and J. Perry on the Magnetic Circuit of Dynamo Machines. (Plate IV.).....	496
Mr. H. F. Newall on the Recalescence of Steel.....	510
Mr. J. Parker on an Extension of Carnot's Theorem.....	512
Notices respecting New Books :—	
Transactions of the Edinburgh Geological Society, Vol. V., Part III. ....	514

	Page
Proceedings of the Geological Society:—	
Report on the Recent Work of the Geological Survey in the North-west Highlands of Scotland .....	515
Mr. W. Barlow on the Horizontal Movements of Rocks .....	517
Mr. J. E. Marr and Prof. H. A. Nicholson on the Stockdale Shales.....	519
Mr. A. Harker on the Eruptive Rocks in the Neighbourhood of Sarn, Caernarvonshire .....	520
On Mountain Formation, by T. M. Reade .....	521
On the Magnetism of Organic Bodies, by S. Henrichsen ....	522
On the Insulating Action of various Substances in respect of Radiant Heat, by H. C. Vogel.....	523
Index .....	524

---

### PLATES.

- I. Illustrative of Mr. E. F. J. Love's Paper on a Method of Discriminating Real from Accidental Coincidences between the Lines of different Spectra.
- II. Illustrative of Mr. C. V. Boys's Paper on Experiments with Soap-bubbles.
- II. Illustrative of Mr. W. E. Sumpner's Paper on the Variation of the Coefficients of Induction.
- IV. Illustrative of Profs. W. E. Ayrton and J. Perry's Paper on the Magnetic Circuit of Dynamo Machines.

Fig. 1.

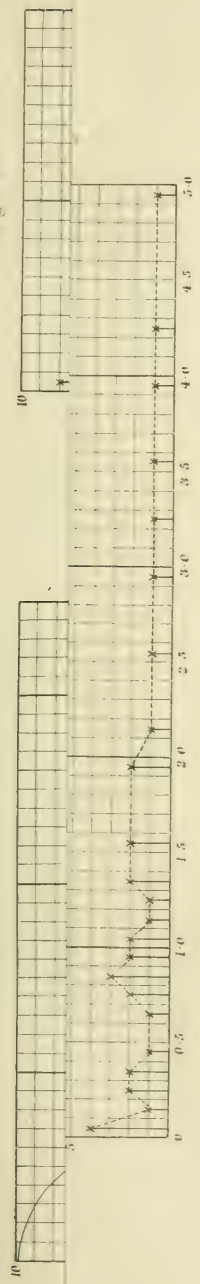


Fig. 2.

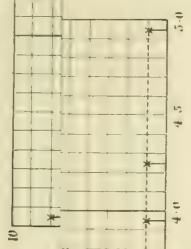


Fig. 6.

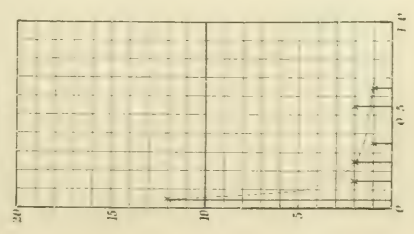


Fig. 7.

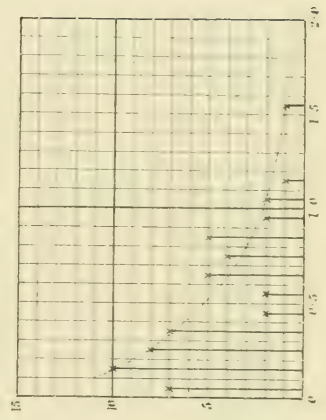
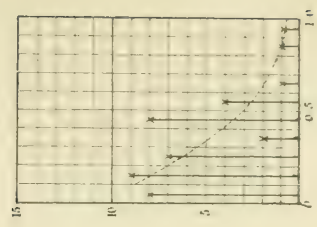


Fig. 8.



Master 5.22.50





THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

---

[FIFTH SERIES.]

---

JANUARY 1888.

---

- I. *On a Method of Discriminating Real from Accidental Coincidences between the Lines of different Spectra; with some Applications.* By E. F. J. LOVE, M.A., Demonstrator of Physics in the Mason College, Birmingham.\*

[Plate I.]

IN investigating problems the solution of which depends on the coincidence of the lines belonging to different spectra, *e. g.*, in determining the presence of any particular substance in the Sun's reversing layer, it is usually considered sufficient to demonstrate a close agreement between the wave-lengths of a number of lines in the spectra. That this method, however, taken by itself, does not suffice to give us a reliable result was shown by Schüster†, who demonstrated that, in accordance with the Theory of Probability, a certain number of coincidences between the lines of two spectra might be expected to occur, even if the spectra be quite unrelated; and showed how to calculate the maximum number of coincidences possible on the assumption that no relation exists between them. If no greater number than this is found, the coincidences must be looked upon as probably accidental. The method as given by Schüster is employed to determine whether the lines of a spectrum are harmonically related; but it could obviously be quite as well applied to examine the relations between two different observed spectra. It demands, however, a considerable amount of rather troublesome computation. Further, cases may occur

\* Communicated by the Physical Society; read November 26, 1887.

† Proc. Roy. Soc. xxxi. p. 337, 1881.

in which approximate equality obtains between a great number of lines in two spectra, one or both of which is so crowded with lines that the question arises, What difference of wave-length between the lines is admissible as a coincidence? This difficulty especially meets us when we are dealing with certain parts of the solar spectrum—especially those obtained from portions of the sun's surface near to, or including, a spot; and Schüster's method gives us no hint towards its solution.

While engaged in preparing a report on Grünwald's\* recent investigations into the relation between wave-length and specific volume, the present writer was led to a simple method of comparison, based on the Law of Error. In accordance with this law, the errors of observation of a single quantity group themselves about the mean value of the quantity in such a way that the number of observations in which the errors are less than some small quantity  $x$  is greater than the number in which they lie between  $x$  and  $2x$ , this again is greater than the number between  $2x$  and  $3x$ , and so on; the equation between the number of observations and magnitude of error being, as is well known, of the form

$$y = ae^{-c^2x^2}.$$

Now since the various spectrum-lines of a substance in a given physical condition are connected by an invariable relation, it seems allowable to assume that observations of the several lines in one spectrum may be regarded as different observations of one phenomenon, viz., that spectrum; as a consequence it is here assumed that, if the differences of the wave-lengths of corresponding lines in spectra really due to the same substance, but determined by different observers, and under different conditions (*e.g.*, the substance as examined by one observer being on the earth; and as examined by the other, on the sun) be compared, they will accord with the Law of Error. The method thence derived is as follows:—The differences between the wave-lengths of the lines compared are arranged in groups, each group containing those observations the errors of which lie within certain narrow limits. The number of observations in each group is then plotted as an ordinate of a curve, the average error of the group being the abscissa. If this curve be then compared with the curve given by the Law of Error, any serious divergence from the form of the latter curve is at once made manifest. It should, however, be borne in mind that the Law of Error admits the possibility of errors of every conceivable

\* *Astr. Nachr.* Bd. 117; *Phil. Mag.* [5] xxiv. p. 354, 1887.



magnitude, and assumes the number of sources of error to be practically infinite; as a result we should expect the curves actually obtained to be steeper if anything than the theoretical curve in the portion near the  $y$ -axis.

It is obvious that the method can only be applied to spectra which contain a considerable number of lines, and that measurements of *all* the coincidences observed must be included, otherwise the method will not give a correct result; it might under different circumstances cause us either to underrate or overrate the probability in favour of the coincidence. On the other hand, in dealing with the solar spectrum, with the aid of Ångström's map, it must be borne in mind that the map is very incomplete, many lines being omitted; as a result we must not expect to find *all* the lines of any substance in the map, even if these lines exist in the sun.

We may illustrate the method by means of the curves shown in figs. 1-5 (Plate I.). Fig. 1 is the Theoretical Curve of Error; fig. 2 that actually obtained by comparing the values of 21 lines in the arc-spectrum of iron, observed by Ångström, with their values obtained by Cornu; fig. 3 is the curve obtained on comparing Kirchhoff's measurements of the spark-spectrum of cerium (27 lines) with those of Thalén. The agreement in form between these curves is obvious, and, considering the somewhat small number of lines included, rather striking; the greater steepness of the two latter curves (owing to the finite number of sources of error) is also well marked. Fig. 4 contains a comparison of the arc-spectra of iron and nickel (19 lines) between wave-lengths 4850 and 5890; and fig. 5 a comparison of those of iron and titanium (34 lines) between the same limits. The divergence of these from fig. 1 is, as we should naturally expect, very marked; so far as *any* curves may be said to represent the results, the best would be straight lines nearly parallel to the  $x$ -axis.

Having given these instances as illustrations of the degree of accuracy of the results to be obtained from the method, we will proceed to examine its bearing on two problems of considerable interest: (1) The existence of cerium in the sun; (2) Professor Grünwald's recent investigations.

#### *Cerium in the Sun.*

The existence of cerium in the sun's reversing layer was indicated as probable by Professors Liveing and Dewar\* in 1882; and in the following year the same observers† pub-

\* Proc. Roy. Soc. xxxiii. p. 428, 1882

† Phil. Mag. [5] xvi. p. 401, 1883.

lished a map, extending from F to *b*, showing among others the arc-spectrum of cerium, and the spectra of the lines seen widened in two sun-spots at Greenwich in 1881. The spectrum of cerium exhibits numerous coincidences with widened lines, though comparatively few with lines given in Ångström's map. On the other hand, Messrs. Hutchins and Holden\*, who have re-examined the evidence for the existence of certain substances in the sun, with the aid of photography, remark, "So numerous are the lines [of cerium, molybdenum, uranium, and vanadium] that often on the photographs the total space occupied by them is greater than the space not so occupied. . . Evidently coincidences between these and solar lines cannot fail to occur as matters of chance and therefore prove nothing. One can easily count a hundred or so such coincidences without the slightest conviction that the connexion is other than fortuitous."

As a complete map of the arc-spectrum of cerium has not yet been published, the writer has been obliged to fall back on the evidence given by Liveing and Dewar's map, mentioned above. It is briefly as follows:—Of the 34 lines included in the map 20 coincide with solar lines which are not already assigned or possibly assignable to other metals; of these solar lines 6 only are represented on Ångström's map, and *the whole* have been observed to be widened, most of them very considerably, in sun-spots, though not all in the same spot. This fact by itself considerably strengthens the case for the existence of cerium as a constituent of the sun, since it demonstrates a connexion between these lines. Let us apply the method to these twenty coincidences. The differences between the wave-lengths of the cerium and solar lines are given in the following table:—

Between 0·0 and 0·1 Xth metre, 12 coincidences.

"	0·1	"	0·2	"	"	2	"
"	0·2	"	0·3	"	"	2	"
"	0·3	"	0·4	"	"	1	"
"	0·4	"	0·5	"	"	0	"
"	0·5	"	0·6	"	"	2	"
"	0·6	"	0·7	"	"	1	"

These numbers when plotted give the curve represented in fig. 6. This curve closely resembles figs. 1, 2, and 3, except in respect of its greater steepness; the latter, so far from being an objection, testifies to the fewness of the

\* Proc. Amer. Acad. Arts and Sci. xxiii.; Phil. Mag. [5] xxiv. p. 325, 1887.

sources of error affecting the wave-length measurements. The evidence afforded by the method appears then to supply a substantial confirmation to the reality of the coincidences, so far as the material for investigation goes.

*Professor Grünwald's Investigations.*

The most important verification of his theory put forward by Grünwald is that afforded by the close agreement between the wave-lengths of the lines in the spectrum of water, as deduced by him from those of the hydrogen spectrum, and their values as obtained by observation. So far, 58 of the predicted lines have been observed by Prof. Liveing; and the remainder occur in a part of the spectrum not yet submitted to examination. The wave-lengths are given in Grünwald's\* paper; the differences between the observed and calculated values are given here:—

Between 0·0 and 0·1 Xth metre, 7 coincidences.

„	0·1	„	0·2	„	„	10	„
„	0·2	„	0·3	„	„	8	„
„	0·3	„	0·4	„	„	7	„
„	0·4	„	0·5	„	„	2	„
„	0·5	„	0·6	„	„	2	„
„	0·6	„	0·7	„	„	5	„
„	0·7	„	0·8	„	„	4	„
„	0·8	„	0·9	„	„	5	„
„	0·9	„	1·0	„	„	2	„
„	1·0	„	1·1	„	„	2	„
„	1·1	„	1·2	„	„	1	„
„	1·5	„	1·6	„	„	1	„

These when plotted give the curve fig. 7, which agrees almost exactly with the formula for the Theoretical Curve of Error, as tested by taking points. On the other hand, four of the points do not agree so well with the smooth curve as those in the cases already examined; and until further measurements are obtained, the method only warrants us in asserting that the balance of probability lies on the side of the reality of the coincidences.

The curve shown in fig. 8 is extremely interesting; it is plotted from the comparison, in Grünwald's paper, between the spectrum of one of his hypothetical constituents of hydrogen, termed by him "b," and the nearest solar lines, as given in Ångström's map. The number of lines included is 41. The differences are as follows:—

\* Phil. Mag. [5] xxiv. p. 357, 1887.



Between 0.0 and 0.1 Xth metre, 8 coincidences.

„	0.1	„	0.2	„	9	„
„	0.2	„	0.3	„	7	„
„	0.3	„	0.4	„	2	„
„	0.4	„	0.5	„	8	„
„	0.5	„	0.6	„	4	„
„	0.6	„	0.7	„	1	„
„	0.7	„	0.8	„	0	„
„	0.8	„	0.9	„	1	„
„	0.9	„	1.0	„	1	„

Here again two points are very much out; but an over-estimation of  $\frac{1}{10}$ th of a Xth metre (a quite possible mistake) in reading off Ångström's scale at three different places would replace these two points in the curve; we may therefore fairly consider that the method affords support to Grünwald's hypotheses.

A curious point in connexion with figs. 7 and 8 lies in the fact that in both the first experimental ordinate, instead of being the greatest, is smaller than the second. Is it possible that this may indicate a systematic error in Grünwald's calculations? The probability of this seems increased by an examination of the errors with regard to sign; for, in the comparison of the hydrogen and water spectra, the average positive error (obtained by dividing the sum of all the errors in which the predicted exceeds the observed value by the

number of such errors) is  $\frac{13.6}{22} = 0.6$  Xth metre; while the

average negative error is  $\frac{17.2}{33} = 0.5$  Xth metre. Since a

constant arithmetical error is highly improbable, the only explanation of this seems to be a small systematic difference between Hasselberg's scale of measurement for the hydrogen-spectrum (from which Grünwald's water lines are obtained by halving the wave-lengths of the hydrogen-lines) and Ångström's scale. As it would seem to be very difficult to detect such a difference of scale in any other way, this example adds another to the purposes to which this method may be applied.

II. *On the Mean Height of the Surface-Elevations, and other Quantitative Results of the Contraction of a Solid Globe through Cooling; regard being paid to the existence of a level of no strain, as lately announced by Mr. T. Mellard Reade and by Mr. C. Davison.* By Rev. O. FISHER, M.A., F.G.S.\*

IN an article which was published in the 'Philosophical Magazine' for February last, I calculated the mean height of the elevations which might have been formed by the cooling of a solid earth upon the "too highly favourable" hypothesis that the whole cubical contraction of the crust was thrown into the vertical dimension, and that each elementary shell consequently retained its original horizontal extension, which, rendering it too large to fit its new position due to the contraction of the matter interior to it, caused the compression by which that particular shell contributed its share towards the surface-elevations. Upon summing these, the resulting mean value appeared to be too small to account for the existing elevations having been caused by contraction of the crust through cooling merely.

Mr. T. Mellard Reade had, however, already pointed out what was the truer conception of a cooling globe: but at the time I had not read his book†. Afterwards Mr. C. Davison independently, as I happen to know, arrived at the same conclusion with Mr. Reade: and in the paper in which he communicated it to the Royal Society in April of this year he criticised my February article as "losing its force," in consequence of the hypothesis not according with the more true conception. In the 'Philosophical Magazine' for November I think I have made it clear that, inasmuch as I had based an argument upon the effects of contraction being too small according to a certain hypothesis, my argument could not, as Mr. Davison supposed, "lose its force" in consequence of his having offered a better and truer hypothesis, which led to the conclusion that mine did not make the effects of contraction small enough.

In the same article I promised a calculation of the mean height of the elevations, which could be formed upon the new and better hypothesis. This I now send, suggesting that, before reading it, it would be well to refer to the unmathematical article of November.

We now follow the new hypothesis, that the earth has

\* Communicated by the Author.

† The Origin of Mountain Ranges (London: Taylor and Francis, 1886). Chap. XI.

cooled as a solid, and that there has always been within the crust a level of no strain, below which the elementary spherical shells have tended to be extended, and above it to be compressed; and we make the probable (though not certain) hypothesis that, below the level of no strain, during the process of contraction, the interior sphere remains without vacancies—that is, the substance settles together by what Mr. Reade calls “compressive extension” and Mr. Davison “stretching.” This precludes our applying separately the coefficient of contraction to the vertical and horizontal dimensions throughout that portion; but we may so apply it to the voluminal only, which is *generally* applicable.

Retaining the symbols used by Sir William Thomson, in his paper on secular cooling\*, let

$r$  = the radius of the earth at present, taken at 20,900,800 feet,

$t$  = the time elapsed since the globe solidified,

$V$  = the temperature of solidification,

$x$  = the distance of a spherical shell of elementary thickness  $dx$  from the surface at the time  $t$ ,

$z$  = the distance of the same shell from the centre,

$z'$  = the distance of the same at the time  $t + dt$ ,

$v$  = the temperature of the said shell at the time  $t$ ,

$\theta$  = the fall of temperature of the shell between the time when it first began to be compressed and the time  $t$ ,

$\phi$  = the fall of temperature of the level of no strain,

$E$  = the coefficient of voluminal contraction,

$e$  = the coefficient of linear contraction,

$h$  = the mean height of the surface-elevations.

Two other quantities are involved, which are defined in Sir W. Thomson's paper, wherein he shows that, at the depth  $x$  at the time  $t$ ,

$$v = C + \frac{b}{a} \int_0^x \epsilon^{-\frac{x^2}{a^2}} dx,$$

where  $b$  is a temperature such that

$$b = \frac{V}{\frac{1}{2} \sqrt{\pi}},$$

and  $a$  is a length such that

$$a = 2 \sqrt{\kappa t}$$

= 402832 feet at the present time,

$\kappa$  being the conductivity of the substance expressed in terms of its own capacity for heat.

\* Trans. Roy. Soc. Edinb. vol. xxiii. part 1, p. 157; also Phil. Mag. [4] vol. xxv.; and Thomson and Tait's 'Natural Philosophy,' p. 711.



## I.

*To find the Depth of the Level of no strain.*

The volume of the shell at the depth  $x$  is  $4\pi x^2 dx$ , and it will contract in the interval  $dt$  through

$$E 4\pi x^2 dx \frac{dv}{dt} dt.$$

The whole contraction of the sphere interior to this shell will therefore be

$$E 4\pi \int_x^r (r-x)^2 \frac{dv}{dt} dt dx.$$

So that the volume of the sphere interior to this shell will become, observing that  $\frac{dv}{dt}$  is negative,

$$\frac{4}{3}\pi(r-x)^3 + \frac{3}{3} E 4\pi \int_x^r (r-x)^2 \frac{dv}{dt} dt dx ;$$

and, neglecting  $E^2$ , its radius will be

$$(r-x) + \frac{E}{(r-x)^2} \int_x^r (r-x)^2 \frac{dv}{dt} dt dx.$$

The circumference of the interior sphere will therefore be diminished in the interval  $dt$  by

$$\frac{2\pi E}{(r-x)^2} \int_x^r (r-x)^2 \frac{dv}{dt} dt dx.$$

It is evident that, if the diminution of this circumference is equal to the horizontal contraction of the shell next above it, that shell will neither be compressed nor extended. But the horizontal contraction of that shell will be

$$2\pi e(r-x) \frac{dv}{dt} dt dx.$$

Hence the condition that the shell at  $x$  is situated at the level of no strain will be, since  $E=3e$ ,

$$\frac{3e}{(r-x)^2} \int_x^r (r-x)^2 \frac{dv}{dt} dx = e(r-x) \frac{dv}{dt}.$$

It will be observed that the position of this level of no strain does not depend on the coefficient of contraction, which will divide out.

According as

$$\frac{3e}{(r-x)^2} \int_x^r (r-x)^2 \frac{dv}{dt} dx \begin{matrix} \geq \\ \leq \end{matrix} e(r-x) \frac{dv}{dt},$$

so will the shell be compressed, not strained, or extended.

Now the fundamental partial differential equation for the conduction of heat gives

$$\frac{dv}{dt} = \kappa \frac{d^2v}{dx^2}.$$

Making this substitution, the above becomes

$$\frac{3e}{(r-x)^2} \int_x^r (r-x)^2 \frac{d^2v}{dx^2} dx \geq \leq e(r-x) \frac{d^2v}{dx^2}.$$

If we integrate by parts, so as to raise the index of  $(r-x)^2$ , observing that  $\frac{d^2v}{dx^2}$  may be put  $=0$  when  $x=r$ , we can obtain Mr. Davison's expression No. 2 in the 'Proceedings' of the Royal Society\*.

Putting the members of the above as an equality, the value of  $x$  will give the depth of the level of no strain.

If we integrate  $\frac{d^2v}{dx^2}$  first, we get by parts

$$\begin{aligned} \int_x^r (r-x)^2 \frac{d^2v}{dx^2} dx &= (r-x)^2 \frac{dv}{dx} + \int_x^r 2(r-x) \frac{dv}{dx} dx \\ &= -(r-x)^2 \frac{V}{\sqrt{\pi\kappa t}} \epsilon^{-\frac{x^2}{4\kappa t}} + \int_x^r 2(r-x) \frac{V}{\sqrt{\pi\kappa t}} \epsilon^{-\frac{x^2}{4\kappa t}} dx, \end{aligned}$$

because

$$\frac{dv}{dx} = 0 \text{ when } x=r.$$

But

$$\frac{d^2v}{dx^2} = -\frac{V}{\sqrt{\pi\kappa t}} \epsilon^{-\frac{x^2}{4\kappa t}} \left( -\frac{2x}{4\kappa t} \right).$$

Hence, dividing by  $\frac{-V}{\sqrt{\pi\kappa t}}$ , transposing, and writing  $a^2$  for  $4\kappa t$ , we have for the equation to find  $x$ ,

$$\frac{3}{(r-x)^2} \left\{ (r-x)^2 \epsilon^{-\frac{x^2}{a^2}} - \int_x^r 2(r-x) \epsilon^{-\frac{x^2}{a^2}} dx \right\} = (r-x) \epsilon^{-\frac{x^2}{a^2}} \frac{2x}{a^2}.$$

Multiply by  $\frac{(r-x)^2}{6r}$ , and transpose.

$$\begin{aligned} \frac{(r-x)^2}{2r} \epsilon^{-\frac{x^2}{a^2}} - \frac{a^2}{2r} \int_x^r \left( -\frac{2x}{a^2} \epsilon^{-\frac{x^2}{a^2}} \right) dx &= \frac{(r-x)^3}{6r} \epsilon^{-\frac{x^2}{a^2}} \frac{2x}{a^2} \\ &= \int_x^r \epsilon^{-\frac{x^2}{a^2}} dx. \end{aligned}$$

The term to be integrated on the left-hand side becomes

$$-\frac{a^2}{2r} \left( \epsilon^{-\frac{r^2}{a^2}} - \epsilon^{-\frac{x^2}{a^2}} \right)$$

between the limits.

Let us take  $a$  for the unit of length. Then  $\epsilon^{-r^2}$  may be neglected, and the equation may be written

$$\frac{1}{2r\epsilon^{r^2}} \{ (r-x)^2 + 1 - \frac{2}{3}(r-x)^3 x \} = \int_x^r \epsilon^{-x^2} dx. \quad (\Lambda)$$

The value of  $x$  given by this equation will be the depth of the level of no strain below the surface in terms of  $2\sqrt{\kappa t}$ , or  $a$ , as the unit of length. In order that the left-hand side may be positive, we must have

$$\frac{2}{3}(r-x)^3 x < (r-x)^2 + 1,$$

or

$$\frac{2}{3}(r-x)x < 1 + \frac{1}{(r-x)^2}.$$

The last term is very small. We shall therefore get a close inferior limit for  $x$  if

$$\frac{2}{3}(r-x)x = 1,$$

which is satisfied by

$$x = \frac{r}{2} \pm \frac{\sqrt{r^2 - 6}}{2}.$$

It is evident that the lower sign must be used.

Putting  $a$  at 402832 feet, which corresponds to  $V=7000^\circ\text{F}$ . and the present epoch, we find  $\log r = 1.7150379$  and  $r = 51.8845a$ . This gives as a limit  $x = 0.0287$ , and the large factor in the term in  $(r-x)^3 x$  warns us that  $x$  must not be otherwise than very small.

The values of the definite integral  $\int_0^x \epsilon^{-x^2} dx$  are tabulated for values at the interval of 0.01\*.

$$\begin{aligned} \text{And} \quad \int_x^r \epsilon^{-x^2} dx &= \int_0^r \epsilon^{-x^2} dx - \int_0^x \epsilon^{-x^2} dx \\ &= 0.8862269254 - \int_0^x \epsilon^{-x^2} dx. \end{aligned}$$

We must therefore try values of  $x$  proceeding by intervals of 0.01.

Let  $x = 0.02$ , and call the left-hand side of the equation (A)  $p$ , and the right-hand side  $q$ .

\* Oppolzer, *Lehrbuch zur Bahnbestimmung*, vol. ii. tab. x.

Then  $p_{x=0.02} = 8.0030$

and  $q_{x=0.02}$  is fractional.

Hence 0.02 is too small a value for  $x$ .

Try  $x = 0.03$ ; then

$$p = -0.94933,$$

and

$$q = 0.856236.$$

Consequently this value of  $x$  is too large, and  $x$  must lie between 0.02 and 0.03.

Treating  $p$  and  $q$  as the ordinates of two curves, the value of  $x$  sought is the abscissa at their intersection, and, if that is  $0.03 - \delta x$ ,  $\delta x$  will be given by

$$\delta x = \frac{p+q}{\frac{dp}{dx} + \frac{dq}{dx}},$$

without regard to signs.

$$\text{Now } \log p = \log \left\{ \frac{1}{2}[(r-x)^2 + 1] - \frac{1}{3}(r-x)^3 x \right\} - \log r\epsilon^{x^2}.$$

$$\therefore \frac{\frac{dp}{dx}}{p} = \frac{-(r-x) + (r-x)^2 x - \frac{1}{3}(r-x)^3}{\frac{1}{2}[(r-x)^2 + 1] - \frac{1}{3}(r-x)^3 x} - \frac{r\epsilon^{x^2} 2x}{r\epsilon^{x^2}}.$$

When  $x = 0.03$  this gives

$$\frac{dp}{dx} = -894.$$

Also

$$\frac{dq}{dx} = \epsilon^{-x^2} = \epsilon^{\frac{1}{0.0009}} = 0.99910;$$

whence

$$\delta x = \frac{p+q}{\frac{dp}{dx} + \frac{dq}{dx}} = 0.00202.$$

And  $x = 0.03 - 0.00202 = 0.0280$  in terms of  $a$  as the unit. This is correct to the number of places given.

Now  $a$  at the present time is, on the supposition that the temperature of solidification was  $7000^\circ \text{F.}$ , 402832 feet. The level of no strain is therefore at present at a depth of 11,279 feet, or 2.1361 miles.

To find  $x$  approximately from the equation

$$\begin{aligned} \epsilon^{-\frac{x^2}{a^2}} \left\{ \frac{(r-x)^2}{2r} + \frac{a^2}{2r} - \frac{(r-x)^3 x}{3ra^2} \right\} &= \int_x^r \epsilon^{-\frac{x^2}{a^2}} dx \\ &= \int_0^r \epsilon^{-\frac{x^2}{a^2}} dx - \int_0^x \epsilon^{-\frac{x^2}{a^2}} dx. \end{aligned}$$



In  $\int_0^r \epsilon^{-\frac{x^2}{a^2}} dx$  put  $\frac{x}{a} = z$ . Then  $dx = a dz$ ; and when  $x$  becomes

$r$ ,  $z$  becomes  $\frac{r}{a}$ .

Therefore 
$$\int_0^r \epsilon^{-\frac{x^2}{a^2}} dx = a \int_0^{\frac{r}{a}} \epsilon^{-z^2} dz;$$

which, if  $z > 4$ ,  $= 0.886a$ .

The equation may then be written

$$\epsilon^{-\frac{x^2}{a^2}} \left\{ \frac{(r-x)^2}{2r} + \frac{a^2}{2r} - \frac{(r-x)^3 x}{3ra^2} \right\} = 0.886a - x + \frac{x^3}{3a^2} \&c.$$

Divide by  $r$ , and neglect terms in  $\frac{x^2}{a^2}$  and  $\frac{x^2}{r^2}$ ,

$$\frac{1}{2} - \frac{x}{r} + \frac{a^2}{2r^2} - \frac{1rx}{3a^2} = 0.886 \frac{a}{r} - \frac{x}{r};$$

$$\therefore x = \frac{3a^2}{r} \left( \frac{1}{2} - \frac{0.886a}{r} + \frac{a^2}{2r^2} \right).$$

If we make  $a$  the unit, as before, and neglect the very small term  $\frac{1}{2r^2}$ , this gives

$$x = 0.0279;$$

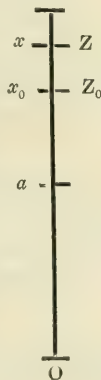
whereas the value obtained by the fuller method was  $x = 0.0280$ , which shows that this is a close approximation.

Hence, generally, the depth of the level of no strain varies nearly as  $a^2$ , that is as the time  $t$ , more nearly as  $mt - nt^3$ .

## II.

Let  $z_0$  be the value of  $z$ , and  $x_0$  of  $x$ , at the level of no strain at any time  $t$ . Then at that time the shells above  $Z_0$  are being compressed, and the coefficient of linear contraction may be applied to them in the horizontal and vertical dimensions separately. But we cannot so apply the coefficient to the shells below  $Z_0$ , where the shells are undergoing what Mr. Reade aptly calls "compressive extension."

From what has already been proved, it appears that the position of the level of no strain does not depend upon the numerical value of the coefficient of contraction; and that, if we neglect  $\frac{x^2}{a^2}$ , its distance from the centre is



$$z_0 = r - \frac{3a^2}{2r},$$

$$= r - \frac{3}{2} \frac{4\kappa t}{r} = r - mt, \text{ suppose.}$$

Hence the depth of this level from the surface is

$$a_0 = \frac{3}{2} \frac{4\kappa t}{r} = \frac{3}{2} \frac{a^2}{r}.$$

This agrees with Prof. Darwin's value,  $\frac{6\kappa t}{r}$ ; but it does not agree with Mr. Davison's law that the depth varies as the square root of the time.  $m$  is very small, being  $\frac{2094}{2000000}$ .

$\frac{d\phi}{dt}$  is the rate at which the temperature at the level of no strain is falling at the time  $t$ . Let  $Z$  be the position of a shell under compression between  $Z_0$  and the surface;  $\frac{d\theta}{dt}$  the rate at which the temperature of  $Z$  is falling.  $O$  the centre of the sphere.

Now the property of the level of no strain gives that the mean contraction of  $OZ_0$  in the interval  $dt$  is the same as that of the circumference at  $Z_0$ , because, on contracting, the shell is neither stretched nor compressed.

Hence, owing to the change in position of that level in the time  $dt$ ,  $OZ_0$  is diminished by  $mdt$ ; and, moreover, by the contraction of the radius according to the property just mentioned, it also diminished by  $e(r-mt)\frac{d\phi}{dt}dt$ .

For like reasons,  $Z_0Z$  is increased by  $mdt$  and diminished by  $\int_{z_0}^z e \frac{d\theta}{dt} dt dz$ .

The  $mdt$ 's cancel; and if we take  $z'$  as the value of  $z$  before the interval  $dt$ , we shall have

$$z' = z + e(r-mt)\frac{d\phi}{dt}dt + e \int_{z_0}^z \frac{d\theta}{dt} dt dz.$$

Now the contribution to the superficial elevations from the shell at  $Z$ , caused by compression during the interval  $dt$  arising from want of room, would be, if the shell did not contract horizontally,

$$(4\pi z'^2 - 4\pi z^2)dz,$$

where it is to be observed that  $z'$  contains  $dt$ .

But, owing to the horizontal contraction of the shell, this

must be diminished by the areal contraction, or by

$$2e \frac{d\theta}{dt} dt 4\pi z^2 dz.$$

Hence the contribution to the surface-elevations in the time  $dt$  from this shell will be

$$4\pi r^2 \frac{d^2 h}{dt dz} dt dz = (4\pi z'^2 - 4\pi z^2) dz - 2e \frac{d\theta}{dt} dt 4\pi z^2 dz.$$

$$\therefore r^2 \frac{d^2 h}{dt dz} = \frac{z'^2}{dt} - \frac{z^2}{dt} - 2ez^2 \frac{d\theta}{dt}.$$

Substituting for  $z'^2$ , and neglecting terms in  $e^2$ ,

$$r^2 \frac{d^2 h}{dt dz} = 2ez(r - mt) \frac{d\phi}{dt} + 2ez \int_{z_0}^z \frac{d\theta}{dt} dz - 2ez^2 \frac{d\theta}{dt}.$$

Since  $z = r - x$ ,

$$dz = -dx;$$

therefore

$$\int_{z_0}^z dz = - \int_{z_0}^z dx = \int_x^{x_0} dx.$$

The equation then becomes

$$\begin{aligned} -r^2 \frac{d^2 h}{dt dx} &= 2e(r - x)(r - mt) \frac{d\phi}{dt} + 2e(r - x) \int_x^{x_0} \frac{d\theta}{dt} dx \\ &\quad - 2e(r - x)^2 \frac{d\theta}{dt}. \end{aligned}$$

We are concerned only with values of  $x$  down to the level of no strain, where  $x$  is about 2 miles; so that the largest value of  $\frac{x}{r}$  is about  $\frac{2}{4000}$ , and terms in  $\frac{x^2}{r^2}$  may be neglected.

Now

$$v = \text{const.} + \frac{b}{\sqrt{4\kappa t}} \int_0^x e^{-\frac{x^2}{a^2}},$$

where  $a^2 = 4\kappa t$ .

We have seen that when  $a$ , or  $\sqrt{4\kappa t}$ , is taken for the unit,

$$x_0 < \frac{1}{\frac{2}{3}(r - x)} + \frac{1}{\frac{2}{3}(r - x)^3};$$

therefore restoring  $a$ ,

$$\frac{x_0}{a} < \frac{a}{\frac{2}{3}(r - x_0)} + \frac{a^3}{\frac{2}{3}(r - x_0)^3}.$$

In the present problem the order of increasing magnitude is  $x$ ,  $x_0$ ,  $a$ , and these all vanish when  $t = 0$ . The above

inequality shows that  $\frac{x_0}{a}$  is zero when the ratio vanishes. *A fortiori* then is the ratio of  $\frac{x}{a}$ , which is always less than that of  $\frac{x_0}{a}$ , a quantity of the order of  $\frac{a}{r}$ , and therefore its square may be neglected. This will reduce the equation of the temperature-curve to

$$v = \text{const.} + \frac{b}{\sqrt{4\kappa t}} x;$$

which signifies that, to the depths with which we are concerned, we have a family of straight lines only to deal with, all starting from the same point on the surface, and becoming more nearly vertical as the time increases. This consideration will facilitate the calculation.

Again,  $\frac{mt}{r}$ , or  $\frac{3}{2} \frac{4\kappa t}{r^2} = \frac{3}{2} \frac{a^2}{r^2}$ , may be neglected when multiplying  $\frac{d\phi}{dt}$ , for it will be found that

$$\int \frac{mt}{r} \frac{d\phi}{dt} dt = \frac{3}{2} b \frac{\sqrt{4\kappa t}}{r^2} x_0.$$

It is also evident that  $\int_x^{x_0} \theta dx$ , when divided by  $r^2$ , may be neglected. Therefore, dividing by  $r^2$ , integrating for  $t$ , and neglecting the small quantities referred to, the equation is reduced to

$$-\frac{dh}{dx} = 2e\phi - 2e\theta + f(x)$$

where  $f(x)$  is to be determined by the limits of  $t$ , the same for  $\phi$  and  $\theta$ . These are, (1) the time when the level of no strain was at  $x$ , and (2) the present time, when it is at the depth  $\frac{3}{2} \frac{a^2}{r}$ . It is obvious that  $\phi$  and  $\theta$  were identically equal at the former epoch; their difference was then 0.

At the second epoch, since we are permitted to regard the temperature-curve as a straight line,

$$\phi = \text{const.} - \frac{b}{a} \frac{3}{2} \frac{a^2}{r};$$

and

$$\theta = \text{const.} - \frac{b}{a} x;$$



$$\therefore -\frac{dh}{dx} = 2e \frac{b}{a} \left( x - \frac{3}{2} \frac{a^2}{r} \right);$$

$$\therefore -h = 2e \frac{b}{a} \left( \frac{x^2}{2} - \frac{3}{2} \frac{a^2}{r} x \right) + C.$$

To obtain the mean height of the total elevations which can be formed out of all the portion of the crust which has been subjected to compression up to the present time, the integral must be taken from 0 to  $\frac{3}{2} \frac{a^2}{r}$ .

Therefore, finally,

$$\begin{aligned} h &= 2e \frac{b}{a} \left( \left( \frac{3}{2} \frac{a^2}{r} \right)^2 - \frac{1}{2} \left( \frac{3}{2} \frac{a^2}{r} \right)^2 \right) \\ &= e \frac{b}{a} \frac{9}{4} \frac{a^4}{r^2}. \end{aligned}$$

In order to fix upon a value for  $e$ , the coefficient of contraction for  $1^\circ$  F., I have reduced the results of some experiments by Mallet upon large masses of slag run from the furnace. They give  $e=0.0000071^*$ . The mean of six results obtained for the contraction of rocks at much lower temperatures by Mr. Adie was  $0.0000057^\dagger$ , and Mr. Mellard Reade's experiments lead to a like result $^\ddagger$ . We may therefore accept  $0.000007$  with tolerable confidence as being at

any rate large enough.  $\frac{b}{a}$  is the temperature-gradient at present, which may be taken at  $\frac{1}{51}^\circ$  F. per foot. The value of  $a$ , corresponding to  $7000^\circ$  F. as the temperature of solidification, is 402832 feet, and  $r$  the radius is 20900800 feet. With these numbers our result gives, for the mean height of all the elevations which would be formed upon a solid earth by cooling,

19 feet.

The smallness of this result renders it impossible to attribute the inequalities of the earth's surface to this cause, even if we confine our attention to the height of the land above the sea, for that alone has been reckoned at 900 feet§. It is also to be observed that, if the level of no strain does not conform to the varying level of the surface, which it probably does, it would not pass beneath the floor of the deeper oceans.

Another very important fact is that the temperature of the

\* 'Physics of the Earth's Crust,' by the Author, p. 68.

† Trans. Roy. Soc. Edinb. vol. xiii. p. 370.

‡ 'Origin of Mountain-Ranges,' p. 112.

§ Nature, vol. v. p. 479.

level of no strain exceeds the surface-temperature by  $b\frac{3}{2}\frac{a}{r}$  degrees only, which, with the high estimate of  $7000^\circ$  for solidification, gives an excess of  $228^\circ$  at present; so that no rock can ever have been pressed up from a depth where its temperature has been much higher than that of boiling water. Neither can it be replied that it may have been otherwise in former ages, because the above expression for the excess varies as the square root of the time and must have been less hitherto than it is now.

The very slight changes, either through cooling or compression, which the rocks above the level of no strain have undergone since solidification, render it improbable that their effects would be observable. The fact that the rocks now being compressed were once extended would not have any effect upon the calculated mean height of the surface-elevations: because their former compressive extension was at the cost of their thickness. If, however, the effects were sufficient, they might be traced petrologically. But it must be remembered that the thickness of the sedimentary strata must be in most places greater than the depth of the level of no strain.

The property of the level of no strain enables us to calculate the contraction of the radius of a solid globe, cooling according to the supposed law; because the mean contraction from the centre to that level is the same as that of the circumference there. Therefore the mean contraction of  $z_0$  in the interval  $dt$  is

$$ez_0 \frac{d\phi}{dt} dt = ez_0 \frac{3}{2} \frac{b}{r} \sqrt{\frac{\kappa}{t}} dt :$$

$\therefore$  in the whole time  $t$  it will be

$$\int_0^t e(r-mt) \frac{3}{2} \frac{b}{r} \sqrt{\frac{\kappa}{t}} dt,$$

or

$$e \frac{3}{2} \frac{b}{r} \sqrt{\kappa} (2rt^{\frac{1}{2}} - \frac{2}{3}mt^{\frac{3}{2}}).$$

The term in  $\frac{m}{r}$  is negligible.

Hence the contraction from the centre to the level of no strain is

$$e \frac{3}{2} b \sqrt{4\kappa t} = e \frac{3}{2} \frac{b}{a} a^2;$$

which, with the assumed values, gives

$$6.3272 \text{ miles.}$$

The vertical contraction of the crust above this level is inconsiderable. It may be thus found :—

$$\begin{aligned} e \int \int \frac{d\theta}{dt} dt dx &= e \int \int \frac{1}{2} \frac{b}{\sqrt{4\kappa}} t^{-\frac{3}{2}} x dt dx \\ &= e \int \frac{1}{2} \frac{b}{\sqrt{4\kappa}} t^{-\frac{3}{2}} \frac{x^2}{2} dt, \end{aligned}$$

which, being taken from  $x=0$  to  $x=mt$ , the depth of the level of no strain,

$$\begin{aligned} &= e \int \frac{1}{2} \frac{bm^2}{\sqrt{4\kappa}} \frac{2}{3} \frac{t^{\frac{3}{2}}}{2} \\ &= \frac{e}{6} \frac{b}{a} \frac{9}{4} \frac{a^4}{r^2} = \frac{1}{6} h = 3 \text{ feet.} \end{aligned}$$

We may therefore say that the

Radial contraction of the globe = 6.3 miles.

It will be noticed that, in the above calculations, no reference has been made to the numerical value of the conductivity, nor to the time elapsed since the surface first assumed its present temperature. All that has been assumed is that the present temperature-gradient, expressed by  $b/a$ , is  $1/51$ , which is about the mean result of innumerable observations; and that the temperature of solidification was  $7000^\circ \text{F}$ . These have been sufficient for our purpose. However, the temperature-gradient being known, and the temperature of solidification assumed, there follows a relation between the time and conductivity; and this appears to have been the original object of Sir William Thomson's investigation. If we make use of the value for the conductivity which he has deduced from certain observations on rocks *in situ* (viz. 400), the results just obtained will correspond to 98 millions of years. But a lower temperature, say  $4000^\circ \text{F}$ ., seems more probable; in which case the time would be about 33 millions of years. In that case all our estimates will have to be reduced. These depend upon the constant  $a$ ; and

$$\begin{aligned} a &= \frac{a}{b} \frac{2V}{\sqrt{\pi}} = 51 \frac{2V}{\sqrt{\pi}}. \\ \therefore \quad a &\propto V. \end{aligned}$$

If, then, we reduce the temperature from  $7000^\circ$  to  $4000^\circ$ ,

$$h' = \left(\frac{4}{7}\right)^4 \times h \text{ feet} = 2 \text{ feet.}$$

The excess of the temperature of the level of no strain over

the surface-temperature  $\propto V^2$ ; so that, if we put the surface-temperature at  $50^\circ$ , it is at present  $124^\circ$  F., and, since it varies as  $\sqrt{t}$ , low as this value may seem, it is higher now than it has ever been.

The depth of the level of no strain is  $\frac{a}{b}$  into the excess of temperature, or  $51 \times 74.56$  feet.

Hence the depth of the level of no strain is now 3802 feet, or about 0.7 of a mile.

The radial contraction  $\propto V^2$ , and will therefore be 2 miles. Lastly,

$$\text{since} \quad h = e \frac{9}{4} \frac{a^3}{r^2},$$

$$h \propto t^{\frac{3}{2}} \quad \text{and} \quad \frac{dh}{dt} \propto \sqrt{t}.$$

This shows that the rate at which mountains would be formed is proportional to the square root of the time, and ought therefore to be greater at present than at any previous period.

Collecting our results, we may say that, on the two suppositions made respecting the temperature of solidification, we should have, at the present time:—

Temperature of Solidification...	7000° F.	4000° F.
Depth of level of greatest cooling*...	54 miles.	31 miles.
Depth of level of no strain .....	2 miles.	0.7 mile.
Temperature of level of no strain ...	258° F.	124° F.
Mean height of elevations ....	19 feet.	2 feet.
Total contraction of radius .....	6 miles.	2 miles.

It must be admitted, from the general bearing of the foregoing results, that the important discovery of the existence of a level of no strain in a solid globe, cooling by conduction, renders more than ever untenable the theory that the surface-elevations can have been formed by the compression of the crust of a solid globe caused in that manner. An inversion of the Horatian line would seem fairly to describe that theory—

*Nascuntur montes, gemit quos ridiculus mus.*

\* See Prof. Darwin's letter, 'Nature,' Feb. 6, 1879.



### III. *A Note on Valency, especially as defined by Helmholtz.*

By HENRY E. ARMSTRONG, *F.R.S.*\*

VERY little has been either said or written of late on the subject of Valency—not because the topic is admitted to be exhausted, nor because our views can be regarded as reposing on a fixed basis of fact, but more I believe on account of the feeling being almost universally entertained that little is to be gained by continuing the discussion from our present standpoint.

My purpose in this note is to call attention to the extreme importance of reopening the discussion on account of the intimate bearing that it has on the work in which the Electrolysis Committee, jointly appointed by this Section and by Section A, are now engaged; and to urge that it is time that the gage thrown down by Helmholtz in the Faraday lecture (*Chem. Soc. Trans.* 1881, p. 277) was uplifted by chemists.

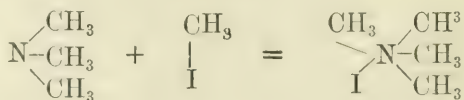
We are told by Helmholtz that it is a necessary deduction from the fundamental law of electrolysis established by Faraday, that definite, as it were atomic, charges of electricity are associated with the atoms of matter; that, in fact, a monad bears a single charge, a dyad two, a triad three; and that when combination occurs, the charges are still retained by the atoms, but neutralize each other—"the atoms cling to their charges, and opposite electric charges cling to each other." I cannot help thinking, however, that Helmholtz deprives his statement of much of its force and simplicity by adding: "But I do not suppose that other molecular forces are excluded, working directly from atom to atom;" he is led to do this apparently by being aware of the distinction which it is usual to draw between atomic and molecular compounds. The attempt should at all events be made—and in my paper on "Residual Affinity" I have already ventured the first step—to include both classes of compounds, molecular as well as atomic, in the discussion; indeed it is somewhat difficult to reconcile the passage above quoted with the following statement which occurs previously in the lecture:—"The law of the conservation of energy requires that the electromotive force of every cell must correspond exactly with the total amount of chemical forces brought into play, not only the mutual affinities of the ions, *but also those minor*

\* Communicated by the Author, being a communication to Section F of the British Association at Manchester.

*molecular attractions produced by the water and other constituents of the fluid.*" The italics are mine. But if the "minor molecular attractions" contribute to the electromotive force of the cell, then conversely these also will have to be overcome in effecting electrolysis, and are as much to be reckoned as are the "mutual affinities of the ions"!

It is obvious that if it should prove possible to decide what number of charges are necessarily associated with any particular atom, the conception of valency will have acquired a definiteness which cannot possibly be attached to it as long as the views that have hitherto guided us are adhered to. A decision must involve the discussion of the question of the existence of molecular as distinct from atomic compounds.

To cast the apple of discord without further preface, I would direct attention to the insufficiency of the evidence on which it is usual to rely as proof that nitrogen, for example, is a pentad; nay more, I would assert that this very evidence should be interpreted as proof that nitrogen is not a pentad. It is commonly held that the behaviour of the alkyl tetra-substituted derivatives of ammonium is such as to negative the idea that these are "molecular compounds" of triad nitrogen, and that it must be assumed that the elements of the binary compound which are added on to the ammonia derivative are *distributed* in the ammonium derivative; for example, that in the formation of tetramethylammonium iodide from trimethylamine and methyl iodide, the methyl and iodine of the iodide part company, and separately attach themselves to the nitrogen, thus:—



But I contend that the properties of tetramethylammonium iodide and hydroxide prove that such is not the case: the iodide, it is well known, can be boiled for hours with the strongest caustic potash solution without undergoing change; there is not a single case on record, however, of any haloid compound other than an alkylic compound behaving in this manner, the chlorides, bromides, and iodides of every element except carbon are almost at once converted into hydroxides by such treatment, and a nitrogen iodide would surely be acted on. The behaviour of the iodine is much more nearly that of iodine in methyl iodide, and, it may be said, exactly that of the iodine in iodobenzene; indeed it would seem that in the alkyl-ammonium haloid compounds the halogen is

always less easily displaced by the action of alkalies than it is in the parent haloid alkylic compound.

The remarkable resemblance of the tetra-substituted ammonium hydroxides to potassium hydroxide has led to their being regarded as in every respect analogous to this latter, and would appear to preclude the idea that they are molecular compounds of an alcohol with an ammonium derivative. But attentive consideration of their properties will suffice, I think, to show that the apparent discrepancies are not only explicable, but that they actually support the molecular compound hypothesis. Thus it might be said to be improbable that tetramethylammonium hydroxide should behave as a powerful base, and have the same heat of neutralization as potassium hydroxide, if methyl-alcohol were one of its proximate constituents; but it is to be remembered that the salt which results from the action of an acid on methyl-alcohol is liable to suffer reconversion into the alcohol by the action of the water produced in the interchange; also that in many cases the methyl salt is insoluble in water, or nearly so. The heat developed on neutralizing methyl-alcohol therefore falls far short in amount of that which would be evolved if the interchange were complete, and if the product were capable of interacting with water, and perhaps also with itself in the way that apparently is possible in the case of metallic salts. In the case of the tetramethylammonium hydroxide, the action of acids is total as the change is irreversible, or almost so, under the conditions which obtain during the formation of the salt, just as in the case of the conversion of potassium hydroxide into a salt; moreover, the product is easily soluble, even when acids like muriatic are used. Why the methyl-alcohol, or other methyl derivative, retained in the ammonium compound behaves so differently as compared with the unassociated methyl derivative, is a question which, for the present, we must be content to put aside unanswered. I am also of opinion that in discussing their constitution, no particular weight can be attached to the mode in which the tetralkylic ammonium hydroxides undergo decomposition when heated, as the products in some cases are an amine and an alcohol, but in others an olefine and water, instead of an alcohol; in the case of the phosphonium salts the diversity is still greater (Chem. Soc. Proceedings, 1886, p. 164). That amines may act as "dehydrating" agents in the manner required, if the molecular compound hypothesis be adopted, appears by no means improbable.

What is here stated of the tetramethyl compounds is true of tetralkylic ammonium haloid compounds generally, in the

sense that they are all less readily acted on by alkalies than are the parent alkylic haloid compounds; but just as these latter are more readily attacked by alkalies and other agents the more complex the alkyl, so are the tetralkyl ammonium compounds; in no case, however, do they manifest a reactivity at all comparable with that of simple metallic or non-metallic haloid compounds—always excepting those of carbon.

The argument used above would apply equally to the phosphonium and sulphine compounds; indeed with greater force.

In many other respects, the behaviour of nitrogen in aminic compounds is altogether peculiar and irreconcilable with the assumption of pentadecity. Thus it is commonly pointed out that the basic properties of aniline, for example, become lessened and ultimately almost annulled by the introduction of chlorine or bromine into the phenyl radical; and that acetamide,  $C_2H_3O.NH_2$ , and other similar compounds formed by the introduction of acid radicals into ammonia are all but destitute of basic properties; the power to form ammonium compounds, therefore, is not a simple function of the nitrogen atom, but is largely dependent on the nature of the radicals associated with the nitrogen atom. Other illustrations are afforded by the hydrazines. Thus phenyl-hydrazine,  $C_6H_5.NH.NH_2$ , although it contains two atoms of (triad) nitrogen, forms with hydrogen chloride the compound  $C_6H_5.N_2H_3.HCl$ , which crystallizes unchanged from fuming muriatic acid, in which, moreover, it is almost insoluble. Ethyl-hydrazine, however, forms a dichlorhydride,  $C_2H_5.N_2H_3.2HCl$ ; but on evaporating the aqueous solution of this salt a monochlorhydride is obtained; and unsymmetric diethyl-hydrazine,  $(C_2H_5)_2N.NH_2$ , is a monobase like phenyl-hydrazine.

Hence it may well be argued that we have no reason to assume that nitrogen is pentad in the ammonium compounds, or phosphorus pentad in the phosphonium compounds, or sulphur tetrad in the sulphine compounds; but that these are all to be reckoned as molecular compounds.

What then is the valency of the elements in question? and what is a molecular compound?

In answer to the first of these questions, the proposition may be advanced that gasefiable hydrogen compounds are the only compounds available for the direct determination of valency, and that the valency of an element—the number of unit charges necessarily associated with its atom—is given by the number of hydrogen atoms combined with the single atom



of the element in its gasefiable hydride\*. In cases where such hydrides are unknown, the determination of valency is very difficult ; it can be but provisionally effected, and only by most carefully weighing all the evidence relating to the constitution of the compounds available for discussion.

But if it be granted, for example, that nitrogen is a triad, and that iodine is a monad, How are we to explain the fact that the methyl compounds of these two elements unite to form so well characterized a molecular compound as tetramethylammonium iodide? How are such molecular compounds constituted? My own view has long been that the nitrogen and iodine in such a case are both possessed of a certain amount of *residual affinity*; and I would define a molecular compound as one formed by the coalescence of two or more molecules, unattended by redistribution of the constituent radicals, and in which the integrant molecules are united by residual affinities. In other words, the unit charge must be capable in certain cases of directly promoting the association, not merely of two, but of at least three, atoms. To put this hypothesis into terms which cannot be misunderstood, let unit valency or charge be represented by a unit line, and further be it supposed that the charge *penetrates the atom*, then the atom with its unit charge may be represented thus :—

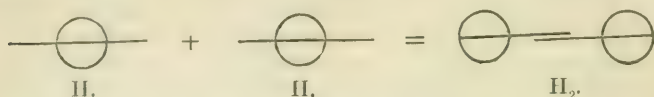


*i. e.*, the unit charge may be held to consist of three portions, the buried portion *a*, and the free portion *f*+*f'*. The facts, as they present themselves to me, also appear to necessitate the assumption that, in the case of different elements, the charge penetrates the atom ; and in the case of some polyad atoms, different directions in the atom with varying degrees of freedom†. The union of two atoms may then be pictured as an overlapping of the unit lines. If the atoms are freely penetrated by their charges, each atom may tend to move out to the end of the line, leaving either no portion, or but a very small portion, free ; a conception of this order would

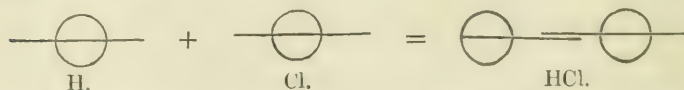
\* If this be granted, it follows that the maximum number of charges which an atom can carry is four ; in other words, that the possible maximum valency is attained in the case of carbon.

† This is practically but a modification of Helmholtz's statement that "the phenomena are the same as if equivalents of positive and negative electricity were attracted by different atoms, and perhaps also by the different values of affinity belonging to the same atom, with different force."

appear to apply in the case of hydrogen, and may be represented thus :—

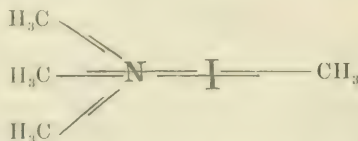


But if the atom be not easily penetrated by its charge, it will not move out to the end of its line, and the resulting compound molecule will possess more or less "residual affinity"; this conception would appear to apply to the non-metals generally, and to some of the metals; it may be illustrated thus :—



I have thought it permissible to state my views in this form merely in order to advance the study of molecular compounds by the introduction of a working hypothesis, an absolutely artificial mode of expression such as is here adopted being perhaps pardonable in the absence of any explanation which may serve to guide us in extending our enquiries as regards the structure of such compounds, a knowledge of which is all important to a rational conception of the nature of chemical change generally. Moreover, I do not hesitate to affirm that, from the chemical point of view, it is impossible to adopt the Helmholtz explanation of valency, unless physicists are prepared to grant the possibility of the "division" of the unit charge somewhat in the manner here suggested; and it is in order to inpress this that I have ventured to give utterance to these speculations.

To return to the consideration of the compounds previously referred to, it may be supposed that the nitrogen of trimethylamine and the iodine of methyl iodide are possessed of residual affinity, and hence the two molecules unite to form the molecular compound tetramethylammonium iodide, which may be represented thus :—



The phosphonium and sulphine iodides may be regarded as similarly constituted. It is well known that the ammonium

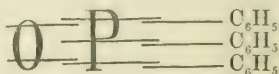
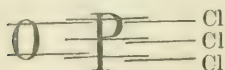
haloid compounds and their analogues are also capable of forming still more complex molecular aggregates with the halogens, &c. : they are therefore to be regarded as possessed of residual affinity ; and that polyad elements, *e. g.* nitrogen, phosphorus and sulphur, should still exhibit residual affinity in such compounds is not surprising in the light of the hypothesis advocated in this note ; but it is scarcely compatible with the assumption that the halogen in the ammonium haloid compounds serves as the bond of union. On the other hand, if it be assumed, as I think it should be, that the formation of double metallic chlorides, &c., is the outcome of the possession of residual affinity by the halogen, the complete analogy which appears to exist between the ammonium haloid compounds and those of the alkali metals would seem logically to involve the inference that the halogen of the ammonium compound does serve as the bond of union. I see but one mode of escape from this conflict of evidence, and that is to call in question the time-honoured assumption that the radical ammonium is the true analogue of potassium and sodium, which, be it remarked, is of necessity subject to doubt if the hypothesis that the ammonium salts are molecular compounds be entertained ; and evidence which supports the conclusion that the per-haloid compound is formed by the addition of the halogen to the nitrogen (phosphorus or sulphur) is afforded by the observation that not only *haloid* ammonium and sulphine compounds, but also the *sulphates* combine with halogens (Dobbin and Masson, Chem. Soc. Trans. 1885, p. 56 ; 1886, p. 846).

It is now proved by abundant experimental evidence that, whatever the order in which the radicals A, B, C, D, are introduced in forming a tetralkylic ammonium compound N(ABCD)X, one and the same end product always results. This is commonly regarded as proof, not only that nitrogen is pentad, but also that the five affinities of the nitrogen atom are of equal value, and it would appear to favour the conclusion that the ammonium salts are in truth "atomic" compounds ; but I see no reason why isomeric change should not occur at the moment of formation of a molecular compound : why the integrant molecules, in fact, should not interchange radicals. If the statement be confirmed that the compound formed from dimethyl sulphide and ethyl iodide is different from that obtained on combining methylethyl sulphide and methyl iodide (Krüger, *Journ. pr. Chem.* 1876, xiv. p. 193), it will follow, not that sulphur is a tetrad, and that the four affinities are of unequal value, but that there is little or no tendency for isomeric change to occur in the formation of

sulphines. The possible occurrence of isomeric change in the formation of molecular compounds, however, is a subject which certainly deserves careful study at the present time.

In the case of phosphorus, the existence of the highly stable gaseous pentafluoride  $\text{PF}_5$ , discovered by Thorpe, is undoubtedly regarded by many as final proof of the pentadecity of this element; but the existence of compounds such as  $\text{H}_2\text{F}_2$ ,  $\text{HFFR}$ , &c., which clearly belong to the class of molecular compounds, is an indication of so marked a tendency on the part of fluorine to combine with itself, that for this reason alone (as Naumann and others have asserted) the pentafluoride is by no means necessarily regarded as an atomic compound. And I would here add that stability affords no criterion as between atomic and molecular compounds, every degree of stability being met with even among those of the former class.

An argument in favour of the pentadecity of phosphorus, which apparently cannot be disposed of by any explanation based on conventional considerations, has, however, been advanced by La Coste and Michaelis (*Berichte*, 1885, p. 2118), who have shown that the compounds obtained from diphenylchlorophosphine,  $\text{PCl}(\text{C}_6\text{H}_5)_2$  and phenol is not identical with the triphenyl-phosphine oxide,  $\text{OP}(\text{C}_6\text{H}_5)_3$ , obtained by oxidizing triphenyl-phosphine, as it should be if the latter were a compound of the formula  $(\text{C}_6\text{H}_5)_2\text{P} \cdot \text{OC}_6\text{H}_5$ ; this last corresponding to the formula  $\text{Cl}_2\text{P} \cdot \text{OCl}$  which has been suggested as that of phosphorus oxychloride, and which appears to derive considerable support from Thorpe's observations on the specific volume of the oxychloride (*Chem. Soc. Trans.* 1880, p. 388). It is, however, conceivable that the oxygen and phosphorus are united by residual affinities, thus:—

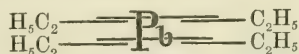


Michaelis and Polis (*Berichte*, 1887, p. 52) have argued in the case of bismuth, which also is a member of the nitrogen group, that the pentadecity of this element is proved by the existence of the triphenyl dibromide  $(\text{C}_6\text{H}_5)_3\text{BiBr}_2$ . But the mere production of such a compound proves nothing so long as its constitution is undetermined; it at most serves to strengthen the conviction gained from the general study of the element, that bismuth is a member of the nitrogen-phosphorus group.

In other cases also it is possible that undue importance



may have been attached to the existence of alkylic compounds of particular types: thus lead, judging from its general chemical behaviour, would appear to be a dyad; yet the existence of the tetrethide  $\text{Pb}(\text{C}_2\text{H}_5)_4$  is commonly held to be a proof that it can function as a tetrad. But the properties of lead are such that I am tempted to suggest that it is one of the metals in which the "charges" have but a small degree of freedom; and it is conceivable that the tetrethide is actually a compound of dyad lead, each charge serving to bind two ethyl groups, thus:—



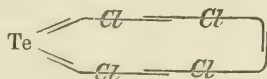
The same may be true of tin, although in this case the fact that we are dealing with an element of the carbon-silicon family tends to favour the conclusion that it may be a tetrad.

Also too much importance must not be attached to the existence of stable volatile chlorine compounds: thus tellurium tetrachloride may well be a compound of dyad tellurium, thus:—



Iron, and the other members of the family which boron heads, in like manner, I feel convinced, are triads even in their *ic* compounds: recent vapour-density determinations all support this conclusion.

It is even conceivable that chlorine may form *closed-chain* compounds, and that a tetrachloride may exist, such as is represented by the formula:—



I think it is especially noteworthy that so many well characterized and comparatively stable double chlorides exist formed by the union of chlorides, of which one at least is *per se* very unstable; the tin-sulphur chloride  $\text{SnCl}_4 \cdot 2\text{SCl}_4$ , and the remarkable series of aurous compounds recently described by Lepetit (*Ann. Chim. Phys.* 1887, p. 11) may be cited as examples.

If my contention in this and previous papers be correct, that residual affinity thus plays a far more important part than has hitherto been supposed, and that it must be taken into account in all discussions on valency, it follows of necessity that our views regarding the constitution of the

majority of compounds at present rest upon a most uncertain basis : the constitution of the paraffins, of the benzenes, and of the haloid compounds and alcohols derived from the hydrocarbons of these series, may be regarded as determined with a degree of precision almost amounting to certainty ; but in the vast majority of other cases we have as yet no secure method of arriving at conclusions which in any sense approach finality. There can be little doubt that in framing our modern conceptions of valency, we have been too much influenced by the graphic symbols which have been so widely made use of. In the future it will be necessary to attach a more liberal interpretation to the facts, and it may be hoped that it will some day be possible also to take into account differences depending on the relation of the different forms of matter to the pervading medium.

IV. *On a Geometrical Determination of the Conditions of Maximum Efficiency in the case of the Transmission of Power by means of Alternating Electric Currents.* By THOMAS H. BLAKESLEY, M.A.\*

1. **T**HIS paper treats the above problem as a particular application of the method of representing Combinations of Electromotive Forces possessing a simple Harmonic Law of Change, already given by the author more than three years ago, and detailed in a series of papers published during 1885 in the 'Electrician.'

It was then pointed out that, though with two constant electromotive forces acting in a simple circuit the transmission efficiency does not exceed the ratio of the smaller to the larger, yet with alternating electromotive forces [denominated by their maximum values] the efficiency may exceed this ratio, owing to the self-induction of the circuit, and to the possibility of varying the interval of time at which the phases of one electromotive force may follow the corresponding phases of the other.

It is here proposed to show how the particular interval of time which will give the maximum efficiency may be geometrically determined, and what the value of that maximum efficiency is for two given electromotive forces undergoing harmonic repetition in the same given period, in a simple circuit possessing a known resistance and a known coefficient of self-

\* Communicated by the Physical Society: read November 12, 1887.

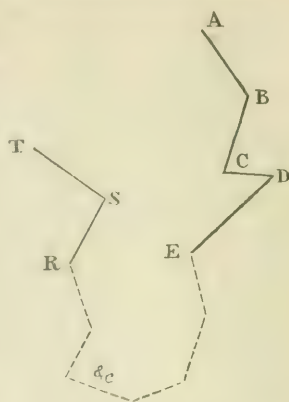
induction. The phase-interval is then the only independent variable in the problem, and what its value must be to give the maximum efficiency of transmission of Power, and what that efficiency will then be, are the questions to which we shall have answers from Geometry alone. A short statement of the general method of representation will make the particular steps required for these problems perfectly clear.

2. Let a straight line of fixed length, and situated in the plane of the paper, undergo uniform rotation in that plane. Then its projection upon a fixed indefinitely long line also in that plane will undergo harmonic variation, and may represent any magnitude capable of undergoing such change (*e. g.* an electromotive force), the maximum value of this varying magnitude being represented by the revolving line itself. The period in which the revolving line makes one complete revolution is the period of the change. Hence, if we know the position of the fixed line and of the revolving line at any instant, we can say in what particular phase the magnitude undergoing harmonic change is at that instant. For instance, suppose these lines make  $30^\circ$  with each other, we can say at once that the magnitude is removed from its maximum value by an interval of time equal to one twelfth of the period. If the angle is at the instant increasing, the magnitude has passed its maximum value that interval of time ago. If the angle is growing less, the magnitude will attain its maximum after that interval of time. It is therefore necessary to fix a positive direction of rotation as representing the positive lapse of time. [That direction which is opposite to that of the hands of a watch will here be adopted.]

3. It follows that when we have two such electromotive forces acting in the same circuit, having different maximum values but the same Period, since each is represented by the projection of a revolving straight line upon a fixed straight line, the resultant electromotive force at the instant is the algebraical sum of the individual projections. And if the two revolving lines are laid down as the two sides of a triangle *taken in order*, the rotation being uniform and the same for both lines, the lines will remain always inclined at the same angle to each other, and the algebraical sum of their projections is the projection of the third side. Thus, in the matter of such electromotive forces, we have a theorem exactly corresponding to the triangle of directed quantities.

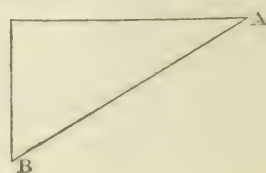
4. We may extend this mode of representing such quantities so as to form a theorem corresponding to the polygon of directed quantities, and cite it thus :—

"If the straight lines AB, BC, CD, ... ST represent the maximum values of different electromotive forces, and, as to direction, are so laid down upon the paper that their projections upon a fixed straight line represent at some point of time the instantaneous values of those electromotive forces, their instantaneous resultant is the projection of the simple straight line AT."



5. If, in any particular case, we have taken into consideration all the electromotive forces concerned, then clearly the line representing the resultant corresponds in phase with the instantaneous current; and if by scaling or calculation we find the value of this resultant in volts, we have only to divide by the resistance in ohms to obtain the maximum value of the alternating current resulting from all the component electromotive forces. This is true even if one of the electromotive forces is that of self-induction. But suppose we have arrived at a preliminary resultant by compounding all the electromotive forces with the exception of that of self-induction; we then require the final resultant, and we obtain it by remembering that it must be at right angles to the electromotive force of self-induction; for the electromotive force of self-induction must be greatest when the current is passing through zero: therefore it must have its projection on the fixed line greatest when that of the final resultant (corresponding with the current) is zero. Therefore the final resultant and the electromotive force of self-induction must be to the preliminary resultant as the two sides of a right-angled triangle including the right angle, are to the hypotenuse; and as we already possess the hypotenuse we have only to determine the ratio of the sides, and upon which side of the hypotenuse they must be placed, in order fully to determine the position and size of the final resultant and the electromotive force of self-induction. The geometrical construction is as follows.

From one end of the preliminary resultant set off an angle in the negative direction of rotation whose tangent is equal to the product of the coefficient of self-induction and the angular velocity of rotation divided by the resistance, and then





complete the right-angled triangle. For if ABC is such a triangle,—AB, BC, AC representing respectively the preliminary resultant, the electromotive force of self-induction, and the final resultant at the maximum values,—it is clear that the maximum rate of increase of the resultant electromotive force will be  $AC \times$  angular velocity. Divide this by the resistance, and the maximum rate of increase in the *current* is obtained, which, multiplied by the coefficient of self-induction, must give the maximum electromotive force of self-induction, from the fundamental conception of that magnitude.

Hence, in symbols, if  $r$  = the resistance,

$L$  = the coefficient of self-induction,

$\omega$  = the angular velocity,

$$BC = \frac{AC}{r} \omega L,$$

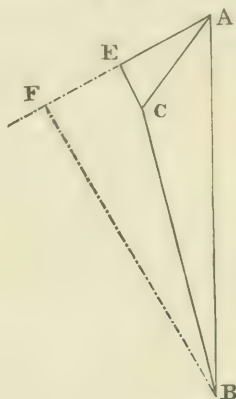
or 
$$\frac{BC}{AC} = \tan BAC = \frac{\omega L}{r}.$$

If  $2T$  is the period,  $\omega = \frac{2\pi}{2T}$ ,  $\therefore \tan BAC = \frac{L\pi}{Tr}.$

And since the electromotive force of self-induction must be greatest and +ve when the current is changing *through zero from +ve to -ve*, it is clear that the phases of the electromotive force of self-induction must *follow* those of the final resultant electromotive force at an interval of time represented by a quarter of the period. Thus the above construction is justified.

6. The power working at any instant in a source of electromotive force is the value of the product of the instantaneous electromotive force in question and of the instantaneous current; but this is constantly changing during a period, and the mean power is half the product of the maximum value of the electromotive force, of the maximum value of the current, and of the cosine of the angle representing the time-interval between their similar phases. I have given a geometrical proof of this theorem in "Alternating Currents." It amounts simply to this in the methods of representation here employed, that if we project the revolving line corresponding to any particular source of electromotive force upon the direction of the final resultant, the power derived from this particular source will be the product of such projection and the final resultant divided by twice the resistance. Hence the various powers of the different sources will simply be proportional to the various projections upon the line of the final resultant.

7. Suppose, then, that AB, BC are the revolving representatives of two electromotive forces. Then AC is their resultant; CAE is an angle whose tangent is equal to  $\frac{L\pi}{T_r}$ , as explained; CE, BF are perpendiculars upon AE. Then AE is the final resultant or effective electromotive force, merely requiring division by the resistance to give the current.



The power derived from the source of AB is  $AF \frac{AE}{2r}$ ; the power transferred to the source of BC is  $FE \frac{AE}{2r}$ ; and the power heating the circuit is  $AE \frac{AE}{2r} = \frac{AE^2}{2r}$ .

As regards the projection of BC, viz. FE, since (as here drawn) FE is in a contrary direction to AE, there is a transfer of power to its source. Had F been situated nearer to A than E is, the source of BC would do work and assist in heating the circuit. This obviously depends upon whether BC, AE are inclined to one another at an angle greater or less than a right angle.

If we denominate these three powers as the power of the active source, the power of the recipient source, and the heating-power, they will be to each other in the proportion

$$AF : FE : AE;$$

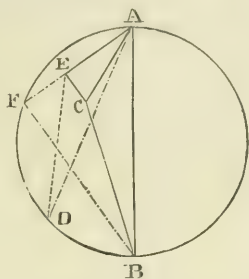
and the efficiency of transmission will be  $\frac{FE}{AF}$ , the ratio of waste being  $\frac{AE}{AF}$ .

8. Since AFB is a right angle, F always lies upon the circle described upon AB as diameter. Describe this circle. We may call it briefly the F circle.

At A set off the angle BAD in the negative direction

equal to CAE, *i. e.* its tangent is equal to  $\frac{L\pi}{T_r}$ , and therefore its magnitude is *independent* of the angle of phase-difference  $\{\pi - ABC\}$ .

Let D be in the F circle, and join DE.



Now it is easy to see that the two triangles EAD, CAB are similar, and their sides homologous in the order of the letters given. For

$$EA = CA \cos EAC,$$

$$DA = BA \cos DAB = BA \cos EAC,$$

and the angle

$$EAD = EAC + CAD = DAB + CAD = CAB.$$

Hence the third side

$$DE = BC \cos EAC,$$

and therefore is invariable, whatever the phase-difference between the lines AB, BC.

But D is also fixed; therefore E lies upon a circle whose centre is D, and whose radius is  $BC \cos EAC$ , or  $BC \cos DAB$ . I call this circle the E circle.

We have now reached the following state of things :—

F must lie upon a fixed circle,

E        "        "        "        "

and

AEF are in one straight line.

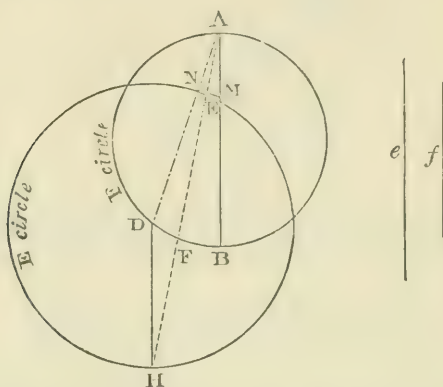
With these prefatory remarks I now proceed to the solution of the questions particularized at the beginning of the paper; which will now be seen to be merely the finding out of the particular position of E upon its circle, which will make the ratio  $\frac{FE}{AF}$  as large as possible, or, which is the same thing,

that of  $\frac{AE}{AF}$  as small as possible.

9. Let  $e$  and  $f$  be the two electromotive forces. Take

AB equal to  $e$ , and cut off from it BM equal to  $f$ . Describe the F circle upon AB, and set off BAD as before, an angle having its tangent equal to  $\frac{L\pi}{Tr}$ .

D is on the F circle. Draw MN perpendicular to AD, and with centre D at distance DN describe a circle. This is the E circle, for its centre is at D, and its radius  $= DN = BM \cos DAB$ .



Through D draw the radius DH parallel to AB, H being on the same side of D that M is of A. Join AH by a straight line cutting the E circle in E and the F circle in F. Then these are the particular positions on the circles which make the ratio  $\frac{FE}{AF}$  a maximum; *i. e.*  $\frac{FE}{AF}$ , as here found, is the maximum efficiency. And if DE be joined, the angle ADE will give the phase-difference between the electromotive forces to give this efficiency.

For, in the first place, it is clear that the ratio  $\frac{FE}{AF}$  has some maximum (not a minimum) value between the positions where E and F coincide, *viz.* when the two circles intersect. And because DH is parallel to AB, and AH cuts them, therefore the angle DHE = the angle BAF.

But DH, BA pass through the centres of the two circles respectively; therefore AH, making the same angle with BA as it does with DH, must cut the circumferences of the E and F circles at the same angle. Thus at E and F, the points where AH cuts the E and F circles respectively, the arcs of those circles are parallel. Therefore an elemental displace-



ment of E and F from these positions would result in no alteration of the ratio in which E divides AF.

But this is the characteristic of a maximum or minimum value of such a ratio. Hence the maximum efficiency is  $\frac{FE}{AF}$ ; and it has before been proved that the triangle ADE is similar to one in which the sides homologous to AD, DE are the electromotive forces themselves. Thus, for maximum efficiency, the phases of  $f$  must follow the phases of  $e$  by an angle equal to  $(180^\circ + ADE)$ , or, which is the same thing, precede them by  $(180^\circ - ADE)$  or  $(\pi - ADE)$ . This, reduced to time, is

$$\frac{(\pi - ADE)}{2\pi} 2T = \left(1 - \frac{ADE}{\pi}\right) T.$$

It is clear that  $\frac{DN}{AD}$  is the ratio of  $f$  to  $e$ . The maximum efficiency  $\frac{FE}{AF}$  exceeds this; and it is seen to do so in virtue of the existence of a coefficient of self-induction, the absence of which would cause D to coincide with B.

*Addendum.—Analytical Expressions.*

The value of the maximum efficiency in symbols is

$$\frac{f}{e} \left\{ \frac{1 + \frac{f}{e} \cos \beta}{\frac{f}{e} + \cos \beta} \right\},$$

where  $\tan \beta = \frac{L\pi}{Tr}$ . The fraction  $\frac{1 + \frac{f}{e} \cos \beta}{\frac{f}{e} + \cos \beta}$  can be easily

shown to be greater than unity. And the angle ADE may be calculated, if desired, as follows:—

where  $ADE = (2\chi - \beta)$ ,

$$\tan \chi = \frac{\sin \beta}{\cos \beta + \frac{f}{e}}.$$

[ $\chi$  is the angle DHA or BAH.]

Adapting to logarithms.

Let

$$\frac{f}{e} = \cos \alpha,$$

$$\tan \chi = \frac{\sin \beta}{2 \cos \frac{\beta + \alpha}{2} \cos \frac{\beta - \alpha}{2}}.$$

There may be transmission of power from the source of  $e$  to the source of  $f$  even when  $f > e$ , provided that  $f \cos \beta$  is not  $> e$ ; as would appear at once from a geometrical construction on the plan given above, and in any case the condition of maximum efficiency is one of stability.

### V. Note on the Motion of a Gas "in Mass."

By Professor TAIT\*.

THE objections raised by Mr. Burbury and Prof. Boltzmann to certain parts of my first paper on the *Kinetic Theory of Gases* were such as I could understand; and my reply to them seems to have been accepted as sufficient.

Mr. Burbury now (Phil. Mag. Dec. 1887) raises an objection, which I do not understand, to an assumption made (after Clerk-Maxwell and Clausius) in my second paper. This objection is based upon a Theorem which Mr. Burbury supposes to have been established by Prof. Boltzmann. The passage objected to is as follows (the *italics* are not in the original):—" . . . . . except in extreme cases, in which the causes tending to disturb the 'special' state are at least nearly as rapid and persistent in their action as is the process of recovery, we are entitled to assume . . . . . that in every part of a gas or gaseous mixture a local special state is maintained. And it is to be observed that this may be accompanied by a common translatory motion of the particles (or of each separate class† of particles) in that region; a motion which, at each instant, may vary continuously in rate and direction from region to region; and which, in any one region, may vary continuously with time. This is a sort of generalization of the special state, and *all that follows is based on the assumption that such is the most general kind of motion which the parts of the system can have, at least in any of the questions here treated.* Of course this translational speed is not the same for all particles in any small part of the system. It is merely an

\* Communicated by the Author.

† The "classes" here spoken of are different kinds of gases, not (as in Mr. Burbury's paper) groups of particles of one gas which have nearly the same speed.

average, which is maintained in the same roughly approximate manner as is the 'special state,' and can like it be assumed to hold with sufficient accuracy to be made the basis of calculation. The mere fact that a 'steady' state, say of diffusion, can be realized experimentally is a sufficient warrant for this assumption; and there seems to be no reason for supposing that the irregularities of distribution of the translatory velocity among the particles of a group should be more serious for the higher than for the lower speeds, or *vice versâ*. For each particle is sometimes a quick, sometimes a slow, moving one:—and exchanges these states many thousand times per second. All that is really required by considerations of this kind is allowed for by our way of looking at the mean free paths for different speeds."

I confess I cannot see how objection can possibly be taken to this assumption when the question is that of the Viscosity of a pure gas; though Mr. Burbury specially mentions Viscosity among the subjects which he considers to be *on this account* erroneously treated in my paper. And it would require, I think, a most determined sceptic to entertain a doubt of its lawfulness in the question of Thermal Conduction, also in a pure gas. In the matter of Diffusion some doubts may possibly occur to one looking at it for the first time; and it was on that account that I inserted in my paper the words quoted above. To deny their validity would, I consider, be tantamount to a wholesale repudiation of the statistical method of treatment, which has done so much for questions of this kind. Mr. Burbury departs from the statistical method in his equation (1) by treating *as a separate gas, with its own pressures and resistances*, the class of particles of one gas which have speeds from  $v$  to  $v + \delta v$ :—thus ignoring the community of interests which the mutual collisions secure for *all* the particles of a gas. It might be lawful, though perhaps not very useful, to treat thus a part of one gas; but it must be a part which contains particles with all varieties of speed in their proper proportions.

But more. I think I have given a self-consistent, and *therefore* accurate, solution of the problem of steady Diffusion (at all events when the masses and the diameters of the particles are the same in the two gases), basing my work entirely on the assumption above. If Mr. Burbury can show that solution to be erroneous, he may possibly make out a presumptive case against the assumption on which it was founded:—but not otherwise.

VI. On Dedekind's Theorem concerning the Motion of a Liquid Ellipsoid under its own Attraction. By A. E. H. LOVE, Fellow of St. John's College, Cambridge\*.

THE interesting problem, to determine the general motion of a mass of liquid in the form of an ellipsoid which moves, subject to its own attraction, in such a way as to remain ellipsoidal and of constant volume, was the subject of a posthumous paper by Dirichlet, edited, with additions, by Dedekind, in Borchardt's *Journal*, vol. lviii. (1859).

Dirichlet set out from the assumption that the coordinates of any liquid particle are at any instant linear functions of their initial values, and showed that all the conditions could be satisfied. The investigations of Greenhill†, completed by Basset‡, lead to the conclusion that the most general type of motion within the ellipsoid can be thus expressed.

Dedekind's most important contribution to the subject is a theorem of reciprocity, according to which there corresponds to any given motion of the ellipsoid a certain other correlated motion. It is the object of this Note to give a proof of this theorem, and especially to discuss the motion which is thus correlated with the well-known Jacobian form of the rotating ellipsoid. This case of motion is usually referred to as "Dedekind's ellipsoid;" and the physical description of it is that the ellipsoidal form remains fixed in space, while the liquid moves about inside it. Regarded from a tidal point of view, we might say that it corresponds to a case of a fixed finite tide with no disturbing body.

Dedekind's Theorem.

Let  $a, b, c$  be the axes of the ellipsoid at any time  $t$ , and  $x, y, z$  the coordinates of a fluid particle at this time referred to axes fixed in space whose origin is at the centre of the ellipsoid;  $a_0, b_0, c_0, x_0, y_0, z_0$  the initial values of these quantities at time  $t=0$ , and let us suppose that at this time the axes of coordinates coincide with the axes of the ellipsoid. Then we take

$$\left. \begin{aligned} x &= x_1 \frac{x_0}{a_0} + y_1 \frac{y_0}{b_0} + z_1 \frac{z_0}{c_0}, \\ y &= x_2 \frac{x_0}{a_0} + y_2 \frac{y_0}{b_0} + z_2 \frac{z_0}{c_0}, \\ z &= x_3 \frac{x_0}{a_0} + y_3 \frac{y_0}{b_0} + z_3 \frac{z_0}{c_0}; \end{aligned} \right\} \dots \dots \dots (1)$$

\* Communicated by the Author.

† Proc. Camb. Phil. Soc. vols. iii. & iv. (1880 and 1882).

‡ Proc. Lond. Math. Soc. vol. xvii. (1886).



where the nine quantities  $x_1, y_1, \dots, z_3$  are functions of the time, and initially  $x_1 = a_0, y_2 = b_0, z_3 = c_0$ , and the rest are zero. Then Dedekind's theorem states that another motion of the ellipsoid will be expressed by the equations

$$\left. \begin{aligned} x &= x_1 \frac{a_0}{a_0} + x_2 \frac{y_0}{b_0} + x_3 \frac{z_0}{c_0}, \\ y &= y_1 \frac{a_0}{a_0} + y_2 \frac{y_0}{b_0} + y_3 \frac{z_0}{c_0}, \\ z &= z_1 \frac{a_0}{a_0} + z_2 \frac{y_0}{b_0} + z_3 \frac{z_0}{c_0}. \end{aligned} \right\} \dots \dots \dots (2)$$

The simplest proof of this is that given by Brioschi, and I here give a version of it.

The kinetic energy of the liquid is T, where

$$2T = \rho \iiint (\dot{x}^2 + \dot{y}^2 + \dot{z}^2) dV; \quad \dots \dots (3)$$

the integration extending through the volume, and  $\rho$  being the density. In virtue of the equation of continuity, which is

$$\Delta \equiv \begin{vmatrix} x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \\ x_3 & y_3 & z_3 \end{vmatrix} = a_0 b_0 c_0, \quad \dots \dots (4)$$

2T becomes

$$\frac{4\pi a_0 b_0 c_0}{15} (\dot{x}_1^2 + \dot{x}_2^2 + \dot{x}_3^2 + \dot{y}_1^2 + \dot{y}_2^2 + \dot{y}_3^2 + \dot{z}_1^2 + \dot{z}_2^2 + \dot{z}_3^2). \quad (5)$$

Again, the potential energy due to the attraction of the ellipsoid on itself is  $\frac{4\pi a_0 b_0 c_0}{15} \chi$ , where

$$\chi = 2\pi\gamma\rho a_0 b_0 c_0 \int_0^\infty \frac{d\psi}{\sqrt{(a^2 + \psi)(b^2 + \psi)(c^2 + \psi)}}, \quad \dots (6)$$

and  $\gamma$  is the constant of gravitation.

We have to express  $\chi$  in terms of the  $x_1, y_1, \dots, z_3$ .

Let the free surface at any instant be

$$(a', b', c', f', g', h' \chi xyz)^2 = 1,$$

the equation giving  $a^2, b^2, c^2$  is the discriminating cubic

$$\begin{aligned} \left(1 + \frac{\psi}{a'^2}\right) \left(1 + \frac{\psi}{b'^2}\right) \left(1 + \frac{\psi}{c'^2}\right) &= 1 + (a' + b' + c')\psi \\ &+ (A' + B' + C')\psi^2 + \Delta'\psi^3 = 0, \end{aligned}$$

where  $A', B', C'$  are the minors of  $a', b', c'$  in the discriminant  $\Delta'$  of  $(a'b'c'f'g'h'\chi xyz)^2$ .

Again, solving (1), the equation of the free surface,

$$\left(\frac{x_0}{a_0}\right)^2 + \left(\frac{y_0}{b_0}\right)^2 + \left(\frac{z_0}{c_0}\right)^2 = 1,$$

becomes

$$\frac{1}{\Delta^2} \left[ \left( x \frac{\partial \Delta}{\partial x_1} + y \frac{\partial \Delta}{\partial x_2} + z \frac{\partial \Delta}{\partial x_3} \right)^2 + \left( x \frac{\partial \Delta}{\partial y_1} + y \frac{\partial \Delta}{\partial y_2} + z \frac{\partial \Delta}{\partial y_3} \right)^2 + \left( x \frac{\partial \Delta}{\partial z_1} + y \frac{\partial \Delta}{\partial z_2} + z \frac{\partial \Delta}{\partial z_3} \right)^2 \right] = 1.$$

Here

$$a' + b' + c' = \frac{1}{\Delta^2} \left\{ \left( \frac{\partial \Delta}{\partial x_1} \right)^2 + \left( \frac{\partial \Delta}{\partial x_2} \right)^2 + \left( \frac{\partial \Delta}{\partial x_3} \right)^2 + \left( \frac{\partial \Delta}{\partial y_1} \right)^2 + \left( \frac{\partial \Delta}{\partial y_2} \right)^2 + \left( \frac{\partial \Delta}{\partial y_3} \right)^2 + \left( \frac{\partial \Delta}{\partial z_1} \right)^2 + \left( \frac{\partial \Delta}{\partial z_2} \right)^2 + \left( \frac{\partial \Delta}{\partial z_3} \right)^2 \right\}.$$

Also

$$\begin{aligned} \Delta' &= \frac{1}{\Delta^4} \left[ \left( \frac{\partial \Delta}{\partial y_2} \frac{\partial \Delta}{\partial z_3} - \frac{\partial \Delta}{\partial z_2} \frac{\partial \Delta}{\partial y_3} \right)^2 + \left( \frac{\partial \Delta}{\partial z_2} \frac{\partial \Delta}{\partial x_3} - \frac{\partial \Delta}{\partial x_2} \frac{\partial \Delta}{\partial z_3} \right)^2 + \left( \frac{\partial \Delta}{\partial x_2} \frac{\partial \Delta}{\partial y_3} - \frac{\partial \Delta}{\partial y_2} \frac{\partial \Delta}{\partial x_3} \right)^2 \right] \\ &= \frac{1}{\Delta^2} (x_1^2 + y_1^2 + z_1^2); \end{aligned}$$

so that

$$A' + B' + C' = \frac{1}{\Delta^2} (x_1^2 + y_1^2 + z_1^2 + x_2^2 + y_2^2 + z_2^2 + x_3^2 + y_3^2 + z_3^2).$$

Also

$$\Delta' = \frac{1}{\Delta^2};$$

thus

$$(a^2 + \psi)(b^2 + \psi)(c^2 + \psi) = \Delta^2 + \psi \Sigma_1^3 \left[ \left( \frac{\partial \Delta}{\partial x} \right)^2 + \left( \frac{\partial \Delta}{\partial y} \right)^2 + \left( \frac{\partial \Delta}{\partial z} \right)^2 \right] + \psi^2 \Sigma_1^3 (x^2 + y^2 + z^2) + \psi^3, \quad (7)$$

and we have to substitute this in (6).

By the principle of Least Action, the equations are to be found by making the integral

$$\int_{t_0}^t \left\{ \frac{1}{2} (\dot{x}_1^2 + \dot{y}_1^2 + \dot{z}_1^2 + \dot{x}_2^2 + \dot{y}_2^2 + \dot{z}_2^2 + \dot{x}_3^2 + \dot{y}_3^2 + \dot{z}_3^2) + \chi \right\} dt$$

stationary, subject to the condition  $\Delta = \text{constant}$ .

Hence, taking  $\sigma$  an arbitrary multiplier, the nine equations of motion are of the type

$$\ddot{x}_1 = \frac{\partial \chi}{\partial x_1} + \sigma \frac{\partial \Delta}{\partial x_1}, \quad \dots \dots \dots (8)$$

And since  $\chi, \Delta$  are not altered by interchanging the rows and columns of  $\Delta$ , we have at once Dedekind's theorem. It is assumed that motions of the type (1) exist; this was proved by Dirichlet and Basset.

### Dedekind's *Ellipsoid*.

The motion in Jacobi's ellipsoid, the fluid rotating as if rigid, is expressed by

$$x = x_0 \cos kt - y_0 \sin kt, \quad y = x_0 \sin kt + y_0 \cos kt, \quad z = z_0.$$

The correlated Dedekind's form is given by

$$\left. \begin{aligned} x &= x_0 \cos kt + \frac{a_0}{b_0} y_0 \sin kt, \\ y &= -x_0 \frac{b_0}{a_0} \sin kt + y_0 \cos kt, \\ z &= z_0. \end{aligned} \right\} \dots \dots (9)$$

The free surface is

$$\frac{x^2}{a_0^2} + \frac{y^2}{b_0^2} + \frac{z^2}{c_0^2} = 1.$$

Since the motion is steady we may drop the suffix zero in  $a_0, b_0, c_0$ .

The Lagrangian equations are three, of the form

$$\ddot{x} \frac{\partial x}{\partial x_0} + \ddot{y} \frac{\partial y}{\partial x_0} + \ddot{z} \frac{\partial z}{\partial x_0} - \frac{\partial V}{\partial x_0} + \frac{1}{\rho} \frac{\partial p}{\partial x_0} = 0,$$

where  $\rho$  is the density,  $p$  the pressure, and  $V$  the gravitation-potential; so that

$$V = \pi \gamma \rho \int_0^\infty \frac{d\psi}{\sqrt{\left(1 + \frac{\psi}{a^2}\right) \left(1 + \frac{\psi}{b^2}\right) \left(1 + \frac{\psi}{c^2}\right)}} \left(1 - \frac{x^2}{a^2 + \psi} - \frac{y^2}{b^2 + \psi} - \frac{z^2}{c^2 + \psi}\right)$$

and the pressure

$$p = \text{const.} + \sigma \left(1 - \frac{x_0^2}{a^2} - \frac{y_0^2}{b^2} - \frac{z_0^2}{c^2}\right).$$

With the usual notation,

$$\frac{\partial V}{\partial x} = -A_x, \dots,$$

and with the above values of  $x, y, z$  the equations of motion become

#### 44 *Motion of a Liquid Ellipsoid under its own Attraction.*

$$-k^2 \left( x \cos kt - y \frac{b}{a} \sin kt \right) + \left( Ax \cos kt - By \frac{b}{a} \sin kt \right) - \frac{2\sigma x_0}{a^2} = 0,$$

$$-k^2 \left( x \frac{a}{b} \sin kt + y \cos kt \right) + \left( Ax \frac{a}{b} \sin kt + By \cos kt \right) - \frac{2\sigma y_0}{b^2} = 0,$$

$$Cz_0 - \frac{2\sigma z_0}{c^2} = 0.$$

Thus,

$$p = \text{const.} + \frac{1}{2} C c^2 \left( 1 - \frac{x_0^2}{a^2} - \frac{y_0^2}{b^2} - \frac{z_0^2}{c^2} \right),$$

and substituting for  $\sigma$ , we obtain two equations,

$$\begin{aligned} \frac{x_0}{a^2} [\Lambda a^2 - C c^2 - k^2 a^2] \cos^2 kt + (B b^2 - C c^2 - k^2 b^2) \sin^2 kt \\ + \frac{y_0}{ab} \sin kt \cos kt [A a^2 - B b^2 - k^2 (a^2 - b^2)] = 0, \end{aligned}$$

and

$$\begin{aligned} \frac{y_0}{b^2} [(A a^2 - C c^2 - k^2 a^2) \sin^2 kt + (B b^2 - C c^2 - k^2 b^2) \cos^2 kt] \\ + \frac{x_0}{ab} \sin kt \cos kt [A a^2 - B b^2 - k^2 (a^2 - b^2)] = 0. \end{aligned}$$

which are both satisfied by

$$k^2 = \frac{A a^2 - C c^2}{a^2} = \frac{B b^2 - C c^2}{b^2}. \quad . \quad . \quad . \quad (10)$$

These are the same conditions as those required for Jacobi's ellipsoid.

Another very simple proof can be derived by Greenhill's method. Suppose the liquid occupying an ellipsoidal case to be rotating, as if rigid, about the least principal axis with angular velocity  $\omega$ , and let an equal and opposite angular velocity be imparted to the case. We shall show that Dedekind's condition is the condition that the surface may be free.

The velocity-potential of the motion set up by rotating the case is

$$-\omega xy \frac{a^2 - b^2}{a^2 + b^2}.$$

Thus the velocity-components of the liquid are

$$u = -\omega y - \omega y \frac{a^2 - b^2}{a^2 + b^2},$$

$$v = \omega x - \omega x \frac{a^2 - b^2}{a^2 + b^2},$$

$$w = 0.$$



The Eulerian equations of motion are

$$\begin{aligned}\frac{\partial}{\partial x} \left( V - \frac{p}{\rho} \right) &= -\omega^2 \left( 1 + \frac{a^2 - b^2}{a^2 + b^2} \right) \left( 1 - \frac{a^2 - b^2}{a^2 + b^2} \right) x, \\ \frac{\partial}{\partial y} \left( V - \frac{p}{\rho} \right) &= -\omega^2 \left( 1 + \frac{a^2 - b^2}{a^2 + b^2} \right) \left( 1 - \frac{a^2 - b^2}{a^2 + b^2} \right) y, \\ \frac{\partial}{\partial z} \left( V - \frac{p}{\rho} \right) &= 0.\end{aligned}$$

Giving

$$V - \frac{p}{\rho} + \frac{1}{2} \omega^2 \left[ 1 - \left( \frac{a^2 - b^2}{a^2 + b^2} \right)^2 \right] (x^2 + y^2) = \text{const.}$$

Since  $V = \text{const.} - \frac{1}{2}(Ax^2 + By^2 + Cz^2)$ , the surface

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1$$

can be free if

$$a^2 \left\{ A - \omega^2 \frac{4a^2b^2}{(a^2 + b^2)^2} \right\} = b^2 \left\{ B - \omega^2 \frac{4a^2b^2}{(a^2 + b^2)^2} \right\} = c^2 C,$$

giving

$$\omega^2 \frac{4a^2b^2}{(a^2 + b^2)^2} = \frac{Aa^2 - Cc^2}{a^2} = \frac{Bb^2 - Cc^2}{c^2}, \quad \cdot \quad \cdot \quad (11)$$

which was the condition found before.

As this case of motion is very interesting physically, and has only been discussed in memoirs devoted largely to other objects, I have thought it worth while to give these proofs.

VII. *The Effect produced on the Thermoelectrical Properties of Iron when under Stress or Strain by raising the Temperature to Bright-red.* By HERBERT TOMLINSON, B.A.\*

THE author has already had the pleasure of communicating to the Physical Society† the results of some experiments relating to a certain curious behaviour of iron wire when under stress or strain at a temperature of bright red. It was found that if the metal was under a slight stress either of torsion or of flexure, there was *apparently* a sudden increase of elasticity when, on heating, the critical temperature had been reached; that is to say, if the wire was under a slight twisting stress it suddenly untwisted, and if under a

\* Communicated by the Physical Society; read November 26, 1887.

† Phil. Mag. vol. xxiv. Sept. 1887, p. 256.

slight bending stress it suddenly unbent. Corresponding sudden changes in the opposite direction occurred on cooling. When, however, the wire was permanently strained and relieved from stress the equally sudden changes which ensued at the critical temperature were opposite in direction to the changes produced when the wire was under stress; namely, a permanently twisted wire free from torsional stress would suddenly twist further when, on heating, a temperature of bright red was reached, and a permanently bent wire would suddenly bend further. Likewise the sudden changes which took place on cooling were in opposite directions for stress and permanent strain: the permanently twisted wire suddenly untwisted, and the permanently bent wire unbent.

Those sudden changes were regarded as probably indicating equally sudden changes in the molecular architecture of iron; and experimental evidence was brought forward to prove that the temperature at which the above-mentioned phenomena occurred was not the same as that at which iron suddenly loses its magnetic properties.

Again, Sir W. Thomson\* arrived at the remarkable conclusion that when a permanent strain is left after the withdrawal of the stress producing it, the residual *thermoelectrical* effect is the reverse of the thermoelectrical effect which is induced by the stress and which subsists as long as the stress acts.

The author was led by the above-mentioned considerations to attempt to ascertain how far the thermoelectrical properties of iron when under temporary stress or permanent strain might be affected by raising the temperature to a bright red.

### *Experiment I.*

A piece of well-annealed iron wire one millimetre in diameter was subjected throughout half its length to a great many turns of permanent torsion. It was then supported on the ring of a retort-stand, being insulated from the latter by paper, with the junction of the twisted and untwisted portions in the centre of the ring. The free ends of the iron wire were connected by silk-covered copper wire with the terminals of a delicate reflecting-galvanometer of about 7 B.A. units resistance; the two junctions between the iron and copper being tied together and well wrapped up in tissue-paper, which served also to insulate them from each other. The junction of the twisted and untwisted portions of the iron wire was now heated slightly by a Bunsen's burner; this

\* "Electro-dynamic Qualities of Metals," Phil. Trans. Part IV. 1856.

caused a deflection of the galvanometer, indicating a current from the strained to the unstrained portion through the hot junction, as already found by Sir William Thomson. The flame of the burner, which was placed underneath the junction, was at first kept small, but was afterwards increased by degrees. At about dull red temperature the deflection nearly ceased to increase, but as soon as the wire assumed a bright red colour, the light reflected from the mirror of the galvanometer went off the scale. The adjusting magnet was therefore used to bring the spot of light back on to the scale in the hope of getting a fairly steady deflection; but this was found to be impossible, the light frequently changing its position by fits and starts and sometimes going over to the other side of the scale. The reason of this unsteadiness will be explained later on.

The experiment was repeated with a second piece of the same wire similarly treated, with this difference, however, that after slightly heating the junction to observe the direction of the deflection, a large resistance was introduced into the galvanometer circuit. By this means the variations of the sudden deflection again produced at the temperature of bright red were much diminished, and a moment of fair steadiness was seized to remove the burner and allow the wire to cool. Almost immediately after the removal of the burner the light on the scale, *after pausing slightly*, went suddenly in the opposite direction.

The experiment was repeated some six or seven times with different pieces of the same wire, but always with like results.

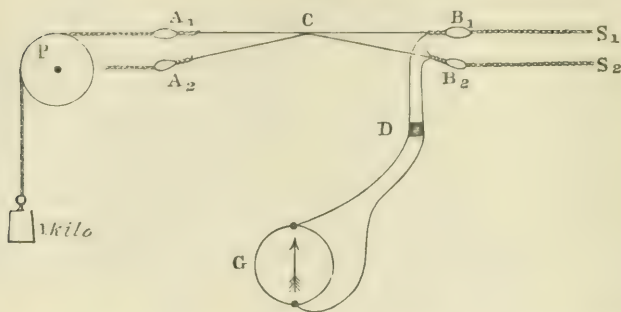
Experiments similar to the above were tried with other pieces of the same iron wire, to which permanent strain was imparted by hammering transversely, by stretching longitudinally, or by bending the wire into coils of small diameter. In all these experiments the thermoelectrical currents produced by moderate heating were, as Sir W. Thomson has already found, from the strained to the unstrained through the hot junction, and in all there was a very sudden increase of the current when, on being heated, the wire became bright red, and an equally sudden decrease when the wire was cooled.

### *Experiment II.*

Two pieces of the same wire were arranged as in fig. 1 (p. 48).  $A_1 B_1$ ,  $A_2 B_2$  are two wires brazed together at C and looped at  $A_1, B_1$  and  $A_2, B_2$ ; from  $B_1$  and  $B_2$  the wires continue to D, where they are joined to the connecting wires of the galvanometer G; D was well wrapped up in tissue-paper.  $B_1 S_1$ ,

$B_2$   $S_2$  are pieces of strong string fastened at  $S_1$ ,  $S_2$  to the ring of a retort-stand weighted at the bottom. From  $A_1$  passes

Fig. 1.



a string over a pulley  $P$ , and to the end of this string a weight of 1 kilo. was attached; from  $A_2$  passes another string to a support so as to hold up the portion  $A_2C$  of the wire  $A_2$ ,  $B_2$ . The two wires  $A_1 B_1$ ,  $A_2 B_2$  lie in the same horizontal plane; to the left of  $C$  they are covered with sand from  $C$  to a distance of 10 centims. To the right of  $C$  for a distance of about 3 centims. the wires are bare, but beyond this for about 7 centims. are covered with sand; the sand is sustained on two stands placed to the right and left of  $C$ .

First, the junction  $C$  was slightly heated by a burner, and as the weight of 1 kilo. was not sufficient for the moderate temperature then used to produce any sensible thermoelectrical effect, the end  $A_1$  was pulled rather hard by the hand to ascertain what would be the direction of the thermoelectrical current due to the stress; this direction was found to be from the unstressed to the stressed wire through the hot junction, as Thomson had already found. A rather large resistance was then introduced into the galvanometer circuit for the purpose of diminishing the sensibility of the instrument, and the parts of the uncovered wires to the right of  $C$  were raised to the temperature of bright red. Immediately this temperature was reached, there was a sudden start of the galvanometer-needles, indicating that the stretched wire had suddenly become more *negative* relatively to the unstretched wire. When the wires were allowed to cool there was, after a short pause, an equally sudden deflection in the opposite direction.

The weight of 1 kilo. was now supported by blocks placed underneath it, and the junction  $C$  was again heated to a bright red. At the critical temperature there was again a sudden deflection, now indicating that the wire, which had



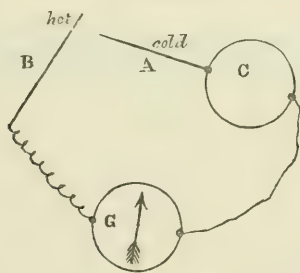
been previously permanently lengthened by the 1 kilo. weight at the bright red temperature, was rendered suddenly more *positive* relatively to the unstrained wire. When the wires were allowed to cool there was, after a short pause, an equally sudden deflection in the opposite direction. This experiment was repeated with three different pairs of wires and with the same results.

It has been mentioned in Experiment I. that as soon as the temperature reached a bright red the deflection of the galvanometer frequently altered by fits and starts, and was even at times strongly reversed in direction. This circumstance constituted a difficulty in all the experiments, and was presently found to be due to slight shiftings of the flame of the burner from slight draughts in the room. Hot iron at any temperature is thermoelectrically negative to cold iron; but when the temperature reaches a bright red there is such a sudden accession of negativeness that if we lay a piece of iron at this temperature upon another piece of cool iron there is developed an electromotive force which, thermoelectrically considered, is very large.

### Experiment III.

In fig. 2 A is a piece of the same iron wire connected with one terminal of a condenser C having a capacity of one third

Fig. 2.



of a microfarad; B is a second piece of the same iron wire connected by insulated copper wire with one terminal of a reflecting-galvanometer G having a resistance of about 6,000 B.A. units; the other terminals of the condenser and galvanometer are connected together with insulated copper wire. The free end of B was heated in a burner nearly to a white heat, and then, having first been removed from the flame, was quickly placed on the free end of A; immediately there was a deflection of the spot of light on the galvanometer-scale

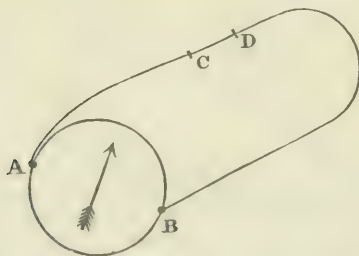
through 10 divisions, showing that the condenser was being charged. Almost immediately after B had touched A, it was removed, and after allowing sufficient time for the wires to cool B was again placed on A, when a deflection through 10 divisions of the scale in the opposite direction indicated the discharge of the condenser. This experiment was repeated several times with fairly uniform results; and by comparing the deflection thus obtained with that produced by a Daniell's cell, it was ascertained that the E.M.F. produced by putting iron at a bright red temperature on iron at about  $15^{\circ}$  C. was one twentieth of a volt, *i. e.* considerably more than twice as much as would be developed in a single element of bismuth and antimony with one junction at  $100^{\circ}$  C. and the other at  $0^{\circ}$  C.

It has long been known that hot iron is negative to cold iron, and this seems to be so at all temperatures, but it has not, the author believes, been noticed before\* how very suddenly the negativeness is increased when the temperature reaches a bright red. If the wire B be heated to a temperature between dull red and bright red before being placed on A, there is a current from A to B which can be readily detected if the condenser be cut out and the galvanometer connected directly with A and B, but no effect whatever could be detected when the condenser was used as in the experiment.

#### *Experiment IV.*

A piece of the same iron wire was connected directly with the two terminals A, B of a reflecting galvanometer of 7 B.A. units resistance (fig. 3). A portion C D of the wire was

Fig. 3.



heated above a bright red by a burner, and as soon as the required temperature had been reached the burner was

\* Unless we except Prof. P. G. Tait's observations, which will be referred to presently.

shifted suddenly and slightly towards C; almost immediately afterwards the light darted off the scale of the galvanometer, indicating a strong current from A to D through C. The portion C D was again heated above a bright red temperature, and the burner then was shifted suddenly towards D; the deflection of the galvanometer needle was now strong in the opposite direction, showing a current from B to C through D.

Experiment III. furnishes the clue to the explanation of the phenomenon described in Experiment IV. The portion C D, when at a bright red, is thermoelectrically negative to the rest of the iron wire, and is in fact like a different metal. So long, however, as the flame of the burner is kept in one position the temperature at C will be the same as at D, and there will be no current; but directly we shift the burner towards C we make C hotter than before, whilst D cools more or less, and we get a current from A to D through C.

We also see why a slight draught of air blowing the flame of the burner towards C or D may suddenly generate a strong current in one direction or the other and may completely mask the phenomena recounted in Experiments I. and II.

Experiment IV. may be varied in several ways, thus:—If we keep the flame steadily burning so as to maintain C D at a high temperature, and allow a small stream of water to flow down on C, we get a continuous current from B to C through D, and if we shift the stream to D we reverse the current. Or, instead of the stream of water, we may blow with a small pair of bellows at C or D.

What connexion there may be between the above-mentioned phenomena and the discovery of Professor P. G. Tait that the thermoelectrical diagram of iron cuts that of iridium-platinum at least *three times* below a low white heat\*, that is to say that an iron and iridium-platinum circuit has at least *three* neutral points, does not quite appear.

In conclusion, it may be said that the investigation serves still further to confirm the author in his opinion that a sudden and profound change takes place in the molecular arrangement of iron at a temperature of bright red.

\* Heat, by P. G. Tait, § 199. Also Trans. R. S. E. 1873, for similar peculiarities in nickel at lower temperatures. The author has failed to observe in nickel wire any such phenomena as those mentioned in Experiments I. and II.

VIII. *On the Theory of Electric Endosmose and other Allied Phenomena, and on the Existence of a Sliding Coefficient for a Fluid in contact with a Solid.* By Professor HORACE LAMB, M.A., F.R.S.\*

THE laws governing the electric transport of conducting liquids through the walls of porous vessels or along capillary tubes, and other related phenomena, have been investigated experimentally by Wiedemann† and Quincke‡, and explained by the latter writer on the assumption of a contact difference of potential between the fluid and its solid boundaries. This explanation has been developed mathematically by von Helmholtz in his well-known paper on electric double layers§. Applying the known laws of motion of viscous fluids, he finds that the calculated results, so far as they depend on quantities which admit of measurement, are in satisfactory agreement with the experiments, and that the values which it is necessary to assign to the contact difference above spoken of are in all cases comparable with the electromotive force of a Daniell's cell. Incidentally he arrives also at the conclusion that in the cases considered there is no slipping of the fluid over the surface of the solids with which it is in contact.

In the present paper a slightly different view is adopted on this latter point. It is assumed that a solid offers a very great, but not an infinite, resistance to the sliding of a fluid over it, and that this sliding is an essential factor in the phenomena referred to. On this modified hypothesis the various cases treated by von Helmholtz are discussed, and in some respects extended. In all cases the results differ from those obtained by von Helmholtz by a factor  $l/d$ , where  $l$  is a linear magnitude measuring the "slip," and  $d$  is the distance between the plates of an air-condenser equivalent to that virtually formed by the opposed surfaces of solid and fluid. For instance, comparing with the experimental results of Wiedemann, von Helmholtz infers that for a certain solution of  $\text{CuSO}_4$  in contact with the material of a porous clay vessel,

$$E/D = 1.77,$$

where  $E$  is the contact difference of potential, and  $D$  the

\* Communicated by the Author: read before Section A of the British Association at the Manchester meeting, September 1, 1887.

† *Pogg. Ann.* lxxxvii. 1852, and xcix. 1856.

‡ *Ibid.* cxiii. 1861. An excellent summary is given in Wiedemann's *Elektricität*, ii. pp. 166 *et seq.*

§ *Wied. Ann.* vii. 1879; or *Collected Papers*, i. p. 855.



E.M.F. of a Daniell's cell. On the views adopted in this paper the inference would be—

$$\frac{E}{D} \cdot \frac{l}{d} = 1.77.$$

Since this involves *two* unknown ratios, no such definite conclusion as to the value of  $E$  can be drawn; but it is evident that the phenomena are consistent even with very small values of  $E/D$ , provided  $l$  be a sufficient multiple of  $d$ . Since this quantity  $d$  is of molecular order of magnitude (comparable probably with  $10^{-8}$  centim.),  $l$  may still be so small that the effects of slipping would be entirely insensible in such experiments as those of Poiseuille.

1. In Wiedemann's experiments the poles of a galvanic battery were connected with two metal plates immersed in a conducting liquid (for instance, copper plates in a solution of  $\text{CuSO}_4$ ) and separated by a porous partition. In one set of experiments the liquid was maintained at the same level on the two sides, and the amount carried by "electric endosmose" through the pores was measured by the overflow on the further side. This amount was found to be proportional to the total amount of electricity conveyed by the current, and independent of the area or of the thickness of the porous partition. For solutions of the same salt, but of different degrees of concentration, the amount of fluid carried across was roughly proportional to the specific electric resistance.

As typical of this class of experiment von Helmholtz considers the case of a straight tube of uniform section, made of insulating material, and containing a liquid through which an electric current is made to flow. Taking the axis of  $x$  parallel to the length of the tube, let  $u$  be the velocity of the fluid at any point,  $\mu$  the coefficient of viscosity,  $\beta$  the coefficient of sliding friction of the fluid in contact with the wall of the tube. Considering the forces acting on a thin surface film, and denoting by  $dn$  an element of the inwardly directed normal, we find—

$$\mu \frac{du}{dn} - \beta u + X = 0, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where the first term is due to the fluid friction on the inner surface of the film, the second to the friction between the outer surface and the tube, while the third term represents the external forces reckoned per unit area. In all ordinary hydrodynamical questions the latter term is absent, but in the present case we have forces due to the fall of potential along the tube, acting on the superficial layer. Let  $E$  be the

excess of potential of the liquid in contact with the wall of the tube over that of the wall itself. It has been pointed out by von Helmholtz that a discontinuity of potential implies the existence, over the surface of discontinuity, of a "double layer" of positive and negative electricity (analogous to the magnetic shells of Ampère), the difference of potential on the two sides being equal to  $4\pi$  times the *electrical moment* of the layer. We therefore suppose that in our present case there exists in a thin superficial stratum of the fluid a distribution of electricity whose amount per unit area is  $\rho$ , say, whilst in a thin superficial stratum of the solid there is a complementary distribution  $-\rho$ . If  $d$  denote (in an obvious sense) the mean distance between these distributions, we have

$$E = 4\pi\rho d,$$

or

$$\rho = cE \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

if

$$c = 1/4\pi d,$$

that is,  $c$  denotes the capacity per unit area of the quasi-condenser formed by the opposed surfaces of solid and fluid. For the case of metallic electrodes (platinum, mercury) in contact with acidulated water, von Helmholtz and Lippmann have independently found the value of  $d$  to be comparable with  $10^{-8}$  centim., and we may reasonably suppose it to be of a similar order of magnitude in the cases at present under consideration.

If  $\phi$  denote the electric potential at any point in the interior of the fluid, we have

$$X = -\rho \frac{d\phi}{dx} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

If  $Q$  be the sectional area of the tube,  $J$  the electric current through it,  $\sigma$  the specific resistance of the liquid, we have, by Ohm's law—

$$-\frac{d\phi}{dx} = \frac{\sigma J}{Q} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

When the motion has become steady, there being no difference of fluid pressure between the two ends of the tube, the velocity  $u$  will be uniform over the section, so that the equation (1) becomes

$$\beta u = \frac{\sigma J}{Q} \rho ; \quad . \quad . \quad . \quad . \quad . \quad (5)$$

and therefore the total flux per second is

$$U = uQ = \frac{\sigma J}{\beta} E \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Since in most cases the flux is in the positive direction of the electric current, we must assume that, as a rule,  $E$  is positive, i. e. the fluid is positive relatively to the solid\*.

To compare with von Helmholtz's result let us write

$$c = 1/4\pi d \quad . \quad . \quad . \quad . \quad . \quad (7)$$

as before, and

$$\mu/\beta = l. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

The constant  $l$ , which is of the nature of a line, measures, as it were, the facility of slipping. In ordinary hydrodynamical problems, in which there is no question of external surface-forces, the surface condition (1) reduces to

$$u = l \frac{du}{dn}. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

The motion will then be sensibly the same as it would be on the hypothesis of no slipping, provided a layer of thickness  $l$  were removed from the surface of the solid and replaced by fluid, it being supposed that  $l$  is small compared with all the dimensions of the space occupied by the fluid.

On making the substitutions (7) and (8), the formula (6) becomes

$$U = \frac{\sigma J}{4\pi\mu} \cdot \frac{l}{d} \cdot E, \quad . \quad . \quad . \quad . \quad . \quad (10)$$

which differs from von Helmholtz's result only in containing the factor  $l/d$ .

In one respect the difference between the view here taken and that adopted by von Helmholtz is little more than verbal. Von Helmholtz considers that the velocity  $u$  is practically uniform over the section of the tube, except near the wall, where it falls rapidly to zero. The stratum within which this fall is supposed to take place is that occupied by the (probably) molecular charges of electricity, whose aggregate is represented by  $\rho$ . The two views might perhaps be reconciled by interpreting von Helmholtz's investigation as virtually a proof that  $l=d$ , if it were not for the assumption that the equations of motion of a viscous fluid, as well as the electrostatic equation

$$\nabla^2\phi + 4\mu\epsilon = 0$$

(where  $\nabla^2 = d^2/dx^2 + d^2/dy^2 + d^2/dz^2$ , and  $\epsilon$  is the volume-density of free electricity), may be supposed to hold through

\* The most noteworthy exception appears to be oil of turpentine in contact with glass or clay. In contact with sulphur, on the other hand, it appears to be positive (Quincke).

the thickness of the stratum in question. Since these equations are only true in a statistical sense, when the linear elements  $dx, dy, dz$  are taken to be large in comparison with the average distance between neighbouring molecules, whereas the thickness of the stratum is almost certainly not more than a very moderate multiple of this distance, it seems doubtful whether they can fairly be pressed into service in the manner indicated.

Although we have only somewhat vague probabilities to guide us, it appears reasonable to suppose, from what we know of contact differences of potential in cases where they can be measured, that the ratio  $E/D$  will not very greatly exceed or fall below unity; that it will lie, say, between about  $\cdot 1$  and  $10$ . If this be so, the comparison of our theory with the observations entitles us to say that the sliding coefficient  $l$  is at all events of the same order of magnitude as  $d$ . If for water in contact with glass  $l$  were equal to  $10^{-8}$  centim., this would make

$$\beta = \mu/l = 1.4 \times 10^6 \text{ C.G.S.};$$

in other words, the shearing-stress necessary (in the absence of electrical surface-forces) to produce a sliding of one centimetre per second would be  $1.4$  megadynes per square centim. It follows that the effects of slipping would be utterly insensible in ordinary hydrodynamical questions, *e. g.* the experiments of Poiseuille. The slipping leads to appreciable results in the cases at present in view, only in consequence of the relatively enormous electrical forces acting on the superficial film, and dragging the fluid (as it were) by the skin, through the tube.

The formulæ (6) may be written:—

$$\frac{\text{Flux of liquid}}{\text{Flux of electricity}} = \frac{\sigma c E}{\beta} \cdot \cdot \cdot \cdot \cdot \quad (11)$$

In this form it can be shown to be true, under a certain restriction, for a tube of varying section, for a network of tubes, and even for the labyrinth of channels contained in the walls of a porous vessel, provided no difference of pressure be allowed to establish itself on the two sides.

Let  $\phi$  denote, as before, the electric potential at any point of the fluid. It will appear that all the conditions of our problem will be satisfied if we suppose the motion of the fluid to be irrotational, the velocity-potential  $\chi$  being everywhere proportional to  $\phi$ .

Since  $\nabla^2 \chi = 0$ , the equations of steady small motion of a viscous liquid, viz.



$$\left. \begin{aligned} -\frac{dp}{dx} + \mu \nabla^2 u &= 0, \\ -\frac{dp}{dy} + \mu \nabla^2 v &= 0, \\ -\frac{dp}{dz} + \mu \nabla^2 w &= 0. \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

are satisfied by  $p = \text{const.}$  To form the boundary condition corresponding to (1), let  $ds$  be a linear element drawn on the surface in the direction of the flow of liquid, and therefore also of electricity. We obtain

$$\mu f - \beta \frac{d\chi}{ds} - \rho \frac{d\phi}{ds} = 0, \quad . \quad . \quad . \quad . \quad . \quad (13)$$

where  $f$  is the rate of shear in a plane through  $ds$  normal to the surface. If  $l$  be small in comparison with the linear dimensions of the channels, the first term of this equation may, in the cases at present under consideration, be neglected in comparison with the rest\*, so that (13) is satisfied provided

$$\chi = -\frac{\rho}{\beta} \phi \quad . \quad . \quad . \quad . \quad . \quad (14)$$

everywhere. Hence the flow of liquid is everywhere in the same direction as that of electricity, and stands to it in the ratio of  $\chi$  to  $-\phi/\sigma$ , that is in the ratio  $\sigma\rho/\beta$ . The formula (11) embraces all the laws discovered experimentally by Wiedemann for the electric transport of liquids through porous vessels.

2. If a difference of pressure obtains between the two sides of a porous wall, or between the two ends of a capillary tube, the flux above calculated must be superposed on that which would be maintained (as in Poiseuille's experiments) by this

\* To see this, take the origin at any point of the boundary, and the axis of  $z$  along the normal, and let the equation to the boundary then be

$$z = \frac{1}{2}(Ax^2 + 2Bxy + Cy^2) + \&c.$$

If the axis of  $x$  be in the direction of the flow at O, we have to prove that  $ld^2\chi/dxdz$  may be neglected in comparison with  $d\chi/dx$ . It is proved in the appendix to this paper that at O we must have

$$\frac{dw}{dx} = Au + Bv,$$

and therefore

$$l \frac{d^2\chi}{dxdz} = lA \cdot \frac{d\chi}{dx},$$

which proves the statement made above, when  $l$  is small in comparison with the radii of curvature of the wall.

difference of pressure in the absence of electrical forces. This follows at once from the linearity of the equations. Wiedemann and Quincke have made experiments in which the fluxes of liquid due to the two causes just balance one another, the subject of measurement being the difference of pressure which exists between the two sides when this equilibrium is established. In Wiedemann's experiments the difference of pressure maintained in this way between the two sides of a porous partition was found to vary directly as the strength of the electric current, inversely as the area of the porous wall, and directly as its thickness. For solutions of different degrees of concentration the pressure was proportional to the electric resistance.

In the case of a tube of uniform circular section treated by von Helmholtz, taking the axis of  $x$  along the axis of the tube, and using cylindrical coordinates  $x, r$ , the first of the equations (12) becomes

$$\frac{dp}{dx} = \mu \left( \frac{d^2 u}{dr^2} + \frac{1}{r} \frac{du}{dr} \right). \quad . \quad . \quad . \quad (15)$$

Here  $p$  is a function of  $x$  only,  $u$  one of  $r$  only. Hence each side of the equation must be constant and  $= P/L$ , where  $L$  is the length of the tube, and  $P$  the difference of pressure between its ends. Hence

$$u = \frac{P}{4\mu L} r^2 + C.$$

Determining  $C$  so that the integral flux across the section is zero, we find

$$u = \frac{P}{4\mu L} \left( r^2 - \frac{R^2}{2} \right). \quad . \quad . \quad . \quad . \quad (16)$$

The velocities close to the wall and in the axis of the tube are equal and opposite. The surface condition, viz.

$$-\mu \frac{du}{dr} - \beta u - \rho \frac{d\phi}{dx} = 0, \quad . \quad . \quad . \quad . \quad (17)$$

leads, since

$$\frac{d\phi}{dx} = - \frac{\sigma J}{\pi R^2},$$

to

$$\begin{aligned} P &= \frac{8\mu L}{\pi R^4 (1 + 4\mu/\beta R)} \cdot \frac{\sigma J}{\beta} \cdot \rho \\ &= \frac{2\sigma J L}{\pi^2 R^4 (1 + 4l/R)} \cdot \frac{l}{d} \cdot E. \end{aligned}$$

If  $A$  denote the E.M.F. along the tube, and if we neglect the small term  $l/R$  in the denominator, we get

$$P = \frac{2A}{\pi R^2} \cdot \frac{l}{d} \cdot E, \quad . \quad . \quad . \quad . \quad . \quad (18)$$

which again differs from von Helmholtz's result only in containing the factor  $l/d$ . The comparison with Quincke's experiments on the discharge of Leyden jars, &c., through a column of liquid in a slightly inclined capillary tube can then be made exactly as in von Helmholtz's paper.

The result contained in (18) can be generalized. Taking, for example, the case of a porous vessel, it has been shown that the flux of liquid due to electrical causes is

$$\frac{\sigma c E}{\beta} \times \text{flux of electricity.}$$

The flux due to the difference of pressure  $P$  on the two sides is  $-P/K$ ,

where  $K$  is a constant depending on the form and arrangement of the channels and on the values of  $\mu$  and  $\beta$ . This constant might be called the "hydraulic resistance" of the system of channels. Equating the total flux of liquid to zero, we find

$$P = \frac{K \sigma c E}{\beta} \times \text{flux of electricity.} \quad . \quad . \quad (19)$$

For a tube of uniform circular section we have, neglecting  $l/R$ ,

$$K = 8\mu L / \pi R^4,$$

leading to our previous result.

3. Quincke has also made observations on the motion of fine particles, suspended in a liquid through which electric currents are flowing. For instance, in the case discussed in § 2, where, under the influence of an electric current, the fluid in a tube of circular section flows (as a rule) forwards along the walls and backwards along the axis, the integral flux across any section being zero, he found, using a glass tube .4 millim. in diameter, that for a certain strength of current the particles near the axis move backwards, whilst those near the walls move forwards, though with less velocity. For stronger currents the motion of the suspended particles is everywhere backwards, but more rapid the nearer to the axis. In narrower tubes the motion was everywhere backwards, even with the feeblest currents which were sufficient to produce perceptible motion at all.

These phenomena have been explained in a general manner by Quinke and von Helmholtz. If  $E$  denote the contact difference of potential between the solid particle and the fluids we have electrifications  $\mp cE$  on the opposed surfaces, which are therefore urged in opposite directions by the electric forces whose components are  $-d\phi/dx$ ,  $-d\phi/dy$ ,  $-d\phi/dz$ .

The principles of this paper lead to a very simple expression for the velocity of an isolated particle when the motion has become steady, viz., the velocity relative to the fluid in this neighbourhood is in the direction of the electric current, and its amount is

$$V = -C\rho/\beta, \quad . \quad . \quad . \quad . \quad . \quad (20)$$

where  $C$  denotes the gradient of electric potential and  $\rho, \beta$ , have the same meanings as before. To prove this, take the axis of  $x$  parallel to the general direction of the electric current in the neighbourhood of the particle. The problem is virtually unaltered if we suppose the fluid to flow with the general velocity  $-V$  past the solid, which is at rest. The electric potential at a distance from the solid will be of the form

$$\phi = -Cx + S_0 + S_{-2} + S_{-3} +, \quad . \quad . \quad . \quad (21)$$

where  $S_0, S_{-2}, S_{-3} \dots$  are solid harmonics of the degrees indicated. These latter terms represent the disturbance of the otherwise uniform flow of electricity by the presence of the insulating solid particles. It will be found that all the conditions of our problem are satisfied by supposing the fluid motion to be irrotational. We therefore write for the velocity potential at a distance

$$\chi = -Vx + T_0 + T_{-2} + T_{-3} +, \quad . \quad . \quad . \quad (22)$$

where  $T_0, T_{-2}, T_{-3} \dots$  are solid harmonics. The surface condition will be of the form (13), in which we may neglect the first term if we suppose the quantity  $l$  defined by (8) to be small in comparison with the dimensions of the particle\*. Hence the condition is satisfied provided

$$\chi = -\frac{\rho}{\beta}\phi; \quad . \quad . \quad . \quad . \quad . \quad (23)$$

and therefore

$$V = -C\rho/\beta. \quad . \quad . \quad . \quad . \quad . \quad (24)$$

In order to satisfy ourselves that the assumption (23) makes

\* For the case of a sphere of a radius  $R$ , I find without making this approximation that

$$V = -\frac{C\rho}{\beta}(1+2l/R).$$



the resultant force and couple on the sphere equal to zero, it will be sufficient to show that the force and couple-resultants of the stress across a closed surface  $\Sigma_1$  drawn in the fluid and just enclosing the solid are zero. Using a common notation for the components of stress at any point of the fluid we have

$$\left. \begin{aligned} p_{xx} &= -p + 2\mu \frac{d^2\chi}{dx^2}, \text{ \&c., \&c.,} \\ p_{yz} &= 2\mu \frac{d^2\chi}{dydz}, \text{ \&c., \&c.} \end{aligned} \right\} \quad . \quad . \quad . \quad (25)$$

where  $p$  is constant, by (12). The resultant stress parallel to  $x$  across the complete boundary  $\Sigma$  of any space occupied by fluid is

$$\iint (lp_{xx} + mp_{xy} + np_{xz}) d\Sigma,$$

where  $l, m, n$  are the direction-cosines of the normal to any element  $d\Sigma$  of the boundary. This surface-integral is equal to the volume-integral

$$\iiint \left( \frac{dp_{xx}}{dx} + \frac{dp_{xy}}{dy} + \frac{dp_{xz}}{dz} \right) dx dy dz$$

taken throughout the interior of  $\Sigma$ , which vanishes, by (25), since  $\nabla^2\chi=0$ . In a similar manner it may be shown that the couple-resultant of the stress across  $\Sigma$  is zero. Now let  $\Sigma$  be made up of the surface  $\Sigma_1$  above defined, and of a sphere  $\Sigma_2$  of infinite radius, having its centre at the origin. It follows that the stresses across  $\Sigma_1$  are statically equivalent to those across  $\Sigma_2$ . And it easily follows from (22) that the latter stresses are in equilibrium.

It is remarkable that the velocity (24) is independent of the size or shape of the particle, so long as its dimensions are large in comparison with  $l$ . This velocity is, of course, to be superposed on that of the fluid in the neighbourhood. For instance, in the circumstances of Quincke's experiments we have

$$C = \frac{\sigma J}{\pi R^2},$$

and, therefore, for a suspended particle of the same nature as the walls of the tube we should have for the absolute velocity the value

$$- \frac{3}{2} \frac{\sigma J}{\pi R^2} \cdot \frac{cE}{\beta}$$

when the particle is in the axis, and

$$-\frac{1}{2} \frac{\sigma J}{\pi R^2} \cdot \frac{cE}{\beta}$$

when it is near the walls\*.

4. We may next consider the electromotive forces produced by the passage of a liquid through a capillary tube or a porous diaphragm. This subject has been studied experimentally by Quincke, Edlund, Haga, Clark, and more recently by Dorn†, the general result being that the potential is higher on the side where the pressure is least by an amount proportional to the difference of pressure. The phenomenon is ascribed to a sort of electric convection, the superficial electrified layer of fluid carrying its charge with it as it slides over the walls of the channels. In the case of a straight uniform tube, for instance, there is in this way a transfer of positive electricity along the walls, from the near to the farther end, which is compensated, if no other path is open, by conduction backwards through the column of liquid in the tube. If the tube be of varying section there will be a tendency also to convergence of positive or negative electricity by convection at intermediate points, and a consequent establishment of "sources" and "sinks" as regards the conducting mass of fluid in the interior.

Taking the case of a tube of circular section, through which fluid is forced by an excess of pressure  $P$ , and using the same notation as in § 2, we find by the ordinary theory of Poiseuille's experiments

$$u = \frac{P}{4\mu L} (R^2 - r^2 + 2\mu R/\beta). \quad \dots \quad (26)$$

Hence the total quantity of electricity carried per second along the wall of the tube is

$$2\pi R \rho \cdot u_R = \frac{\pi R^2}{\beta L} \cdot \rho.$$

If no other conducting channel is open the electricity thus carried forward will return by ordinary conduction through the column of liquid in the tube. Since the resistance of this column is  $\sigma L/\pi R^2$ , the difference of potential between the ends of the tube is

$$\frac{P}{\beta} \cdot cE. \quad \dots \quad (27)$$

\* It is to be noticed that one of Quincke's observations remains unexplained, viz., the fact that in sufficiently wide tubes the direction of motion of particles near the walls varied with the strength of the current.

† For references see Wiedemann, *Elektricität*, ii. pp. 153 *et seq.*

If  $E$  is positive (as it appears to be in most cases) the higher potential is at the end towards which the liquid is forced. With the same substitutions as before, this becomes

$$\frac{P}{4\pi\mu} \cdot \frac{l}{d} \cdot E, \quad . \quad . \quad . \quad . \quad . \quad (28)$$

differing from von Helmholtz's result by the factor  $l/d$ , as in the previous cases\*.

This result does not involve the dimensions of the tube, and may therefore be surmised, like that contained in (10), to be of much wider application than to the particular form of channel above considered. It may be shown, in fact, that if a liquid is forced by pressure through any system of channels with homogeneous walls, and no external path is provided for the electricity set free at various points of these, the resulting distribution of electric potential is given by

$$\phi = -\frac{\sigma\rho}{\beta}p + \text{const.} \quad . \quad . \quad . \quad . \quad (29)$$

In the first place it follows from (12) that this value of  $\phi$  satisfies  $\nabla^2\phi=0$ .

We have next to take account of the fact that the integral amount of electricity which, in consequence of the slipping of the superficial film of liquid, crosses the contour of any elementary area  $dS$  of the wall is not in general accurately zero, and that each such element  $dS$  must be regarded, in relation to the conducting mass of liquid, as a (positive or negative) "source" of electricity. If the origin be taken in this element, and the axis of  $z$  normal to it, the strength of this source is

$$-\rho\left(\frac{du}{dx} + \frac{dv}{dy}\right)dS$$

or

$$\rho\frac{dw}{dz}dS.$$

Now at the origin we have

$$\left. \begin{aligned} w &= 0 \\ u &= l\left(\frac{du}{dz} + \frac{dw}{dx}\right) \\ v &= l\left(\frac{dv}{dz} + \frac{dw}{dy}\right) \end{aligned} \right\} . \quad . \quad . \quad . \quad . \quad (30)$$

\* Dorn infers from a comparison of his experimental results with von Helmholtz's formula that for water in contact with the glass of his tubes  $ED=3.9$ , about.

and if  $l$  be small in comparison with the radii of curvature of the walls &c., we may neglect the second terms in the brackets\*. Under the same circumstances we shall also have, approximately,

$$\left. \begin{aligned} \frac{du}{dx} &= l \frac{d^2u}{dz dx} \\ \frac{dv}{dy} &= l \frac{d^2v}{dz dy} \end{aligned} \right\} \dots \dots \dots (31)$$

so that the expression for the strength of the "source" becomes

$$-\rho l \left( \frac{d^2u}{dx dz} + \frac{d^2v}{dy dz} \right),$$

or

$$\rho l \frac{d^2w}{dz^2}.$$

We may further neglect  $d^2w/dx^2$ ,  $d^2w/dy^2$  in comparison with  $d^2w/dz^2$ , so that the last expression may be written

$$\rho l \nabla^2 w,$$

which equals

$$\frac{\rho l}{\mu} \frac{dp}{dz}$$

by (12). Hence (29) makes

$$\text{source} = -\frac{1}{\sigma} \frac{d\phi}{dz},$$

which is the proper surface condition for  $\phi$ .

5. A similar investigation applies to the electromotive forces called into play by the motion of solid particles through a liquid. This phenomenon, which is in a sense the converse of that discussed in § 3, has been observed by Dorn in the case of grains of sand or glass beads descending by gravity through a vertical column of water. For the case of steady motion the formula (29) shows that the top of the column will be at a higher potential than the base by an amount equal to  $\sigma\rho/\beta$  times the pressure per unit area of the base due to the solid particles. This pressure is equal to the effective weight (*i. e.*, the gravity *minus* the buoyancy) of the particles vertically over the unit area. In Dorn's experiments the observed excess of potential was in fact positive, in accordance with the general rule that  $\rho$  (and therefore  $E$ ) is positive, but the data are not sufficient for further comparison with theory.

\* The justification of these and the following approximations is given in the Appendix.



The details of the process may be illustrated by the case of a spherical particle. If  $r$  denote the distance from the centre,  $\theta$  the angular distance from the lowest radius, the stream-function for the relative motion is of the form

$$\Psi = \left( \frac{A}{r} + Br - \frac{1}{2} V r^2 \right) \sin^2 \theta, \quad . \quad . \quad . \quad (32)$$

where  $V$  is the velocity of the sphere. The relative velocity of the fluid over the surface is therefore

$$\Theta = - \frac{1}{r} \frac{d\Psi}{dr} = \left( \frac{A}{R^3} - \frac{B}{R} + V \right) \sin^2 \theta, \quad . \quad (33)$$

if  $R$  be the radius. In consequence of the slipping, the zone bounded by  $\theta$  and  $\theta + d\theta$  gains electricity at the rate

$$-\rho \frac{d}{d\theta} (2\pi R \sin \theta \cdot \Theta) d\theta.$$

Dividing by the area  $2\pi R^2 \sin \theta \cdot d\theta$  of the zone, we find that each point of the spherical surface is, in regard to the surrounding conducting mass, a source of electricity of strength

$$- \frac{2}{R} \left( \frac{A}{R^3} - \frac{B}{R} + V \right) \rho \cos \theta$$

per unit area. Now

$$\left. \begin{aligned} A/R^3 &= -\frac{1}{4} V / (1 + 3l/R) \\ B/R &= \frac{3}{4} V (1 + 2l/R) / (1 + 3l/R) \end{aligned} \right\}, \quad . \quad . \quad (34)^*$$

whence, for the strength of the source,

$$- \frac{3V}{R^2} l \rho \cos \theta, \quad . \quad . \quad . \quad . \quad . \quad (35)$$

approximately. The corresponding potential at any point of the fluid is therefore of the form

$$\phi = \frac{C \cos \theta}{r^2} + \text{const.}, \quad . \quad . \quad . \quad . \quad (36)$$

\* "Motion of Fluids," § 185. I take occasion to correct the final result (46) of the article referred to. The dissipation of energy by sliding friction has been overlooked. Allowing for this I now find, in the notation there employed,

$$P = 6\mu a V \cdot (1 + 2\mu/\beta a) / (1 + 3\mu/\beta a).$$

If  $\mu/\beta a (=l/a)$  be small, this is equal to the resistance which would be experienced by a sphere of radius  $a - l$  in the absence of slipping.

*Phil. Mag.* S. 5, Vol. 25, No. 152, Jan. 1888. F

with the condition that at the surface

$$-\frac{1}{\sigma} \frac{d\phi}{dr} = -\frac{3V}{R^2} l\rho \cos \theta;$$

whence

$$C = -\frac{3}{2}\sigma VRl\rho. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (37)$$

If we neglect the slipping, the hydrodynamical theory gives

$$\rho = \frac{3}{2}\mu VR \cdot \frac{\cos \theta}{r^2} + \text{const.}, \quad . \quad . \quad . \quad . \quad . \quad (38)$$

so that the relation (29) is verified.

6. It is to be noticed that a comparison of the results of § 1 with those of § 4 indicates the existence of a Dissipation-Function; and from this point of view the connexion between the various classes of phenomena discussed in this paper may be very concisely exhibited. Considering, for instance, the case of a porous diaphragm, and distinguishing the two sides of it by the letters A and B, let P be the excess of pressure, and V that of electric potential, in the fluid on the side A. If U be the quantity of fluid, J that of electricity, which is transferred per second from A to B, then the rate of dissipation of energy is

$$2F = PU + VJ. \quad . \quad . \quad . \quad . \quad . \quad . \quad (39)$$

Now P and V are obviously linear functions of U and J, say

$$\left. \begin{aligned} P &= KU + \kappa J \\ V &= \lambda U + RJ \end{aligned} \right\}, \quad . \quad . \quad . \quad . \quad . \quad (40)$$

where K is the hydraulic and R the electric resistance of the system of channels. In the case of § 1 we have  $P=0$ , and therefore

$$U = -\frac{\kappa}{K} J,$$

whilst in § 2,  $U=0$ , and therefore

$$P = \kappa J.$$

Again, in the case of § 4 we have  $J=0$ , and therefore

$$V = \lambda U = \frac{\lambda}{K} \cdot P.$$

The results we have obtained show that

$$\kappa = \lambda = -K\sigma\rho/\beta. \quad . \quad . \quad . \quad . \quad . \quad (41)$$

Hence we have

$$\left. \begin{aligned} P &= \frac{dF}{dU} \\ V &= \frac{dF}{dJ} \end{aligned} \right\} \dots \dots \dots (42)$$

where

$$F = \frac{1}{2} KU^2 - \frac{K\sigma\rho}{\beta} UJ + \frac{1}{2} RJ^2; \quad \dots \dots (43)$$

that is,  $F$  possesses the characteristic property of a dissipation-function\*. If we had been entitled, *a priori*, to assert the existence of such a function, the laws of the phenomena considered in § 4 could have been deduced from those of § 1.

If the suffixes  $_1$  and  $_2$  refer to the circumstances of two different experiments, we have

$$P_1 U_2 + V_1 J_2 = P_2 U_1 + V_2 J_1. \quad \dots \dots (44)$$

In particular if  $P_1 = 0, J_2 = 0$ ,

$$\frac{V_2}{P_2} = -\frac{U_1}{J_1}, \quad \dots \dots \dots (45)$$

as is otherwise evident from (41) and the preceding equations.

I do not know whether experiments on the electric transfusion of liquids through a porous diaphragm, and on the electromotive forces developed by difference of pressure between the two sides, have ever been made with the same apparatus. In any future experiments on these subjects, the testing of the reciprocal relation (45) would be of interest, and would apparently not present any great difficulty.

#### APPENDIX.

I give here the proofs of certain relations which hold between the fluid velocities  $u, v, w$ , and their space-derivatives at any point of a rigid boundary. Some of these have been employed in §§ 1 and 4.

Taking the origin on the boundary, and the axis of  $z$  along the normal, let the equation to the boundary be

$$z = \frac{1}{2}(Ax^2 + 2Bxy + Cy^2) + \frac{1}{6}(Fx^3 + 3Gx^2y + 3Hxy^2 + Ky^3) + \dots (46)$$

Let us first express the kinematical condition that the velocity in the direction of the normal is zero at all points of the wall. The direction-cosines of the normal at any point  $(x, y)$  near

\* See Rayleigh's 'Sound,' i. § 81.

the origin are

$$\left. \begin{aligned} &-(Ax + By) - \frac{1}{2}(Fx^2 + 2Gxy + Hy^2) \\ &-(Bx + Cy) - \frac{1}{2}(Gx^2 + 2Hxy + Ky^2) \\ &1 - \frac{1}{2}(Ax + By)^2 - \frac{1}{2}(Bx + Cy)^2 \end{aligned} \right\} \quad . \quad . \quad (47)$$

approximately. The condition in question therefore is

$$\begin{aligned} &-\left\{ Ax + By + \frac{1}{2}(Fx^2 + 2Gxy + Hy^2) \right\} \left\{ u + \frac{du}{dx}x + \frac{du}{dy}y + \dots \right\} \\ &-\left\{ Bx + Cy + \frac{1}{2}(Gx^2 + 2Hxy + Ky^2) \right\} \left\{ v + \frac{dv}{dx}x + \frac{dv}{dy}y + \dots \right\} \\ &+ \left\{ 1 - \dots \right\} \left\{ w + \frac{dw}{dx}x + \frac{dw}{dy}y + \frac{1}{2}\frac{dw}{dz}\left(Ax^2 + 2Bxy + Cy^2\right) \right. \\ &\quad \left. + \frac{1}{2}\left(\frac{d^2w}{dx^2}x^2 + 2\frac{d^2w}{dx\,dy}xy + \frac{d^2w}{dy^2}y^2\right) + \right\} = 0, \end{aligned}$$

where the symbols  $u, v, w$ , &c., denote the values of these quantities at the origin. It follows that

$$w = 0,$$

$$\left. \begin{aligned} \frac{dw}{dx} - Au - Bv &= 0 \\ \frac{dw}{dy} - Bu - Cv &= 0 \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (48)$$

$$\left. \begin{aligned} \frac{d^2w}{dx^2} + A\frac{dw}{dz} - Fu - Gv - 2A\frac{du}{dx} - 2B\frac{dv}{dx} &= 0, \\ \frac{d^2w}{dx\,dy} + B\frac{dw}{dz} - Gu - Hv - A\frac{du}{dy} - B\left(\frac{du}{dx} + \frac{dv}{dy}\right) - C\frac{dv}{dx} &= 0, \\ \frac{d^2w}{dy^2} + C\frac{dw}{dz} - Hu - Kv - 2B\frac{du}{dy} - 2C\frac{dv}{dy} &= 0. \end{aligned} \right\} \quad (49)$$

Take next the dynamical boundary conditions. At the origin these are\*

$$\left. \begin{aligned} u &= l\left(\frac{du}{dz} + \frac{dw}{dx}\right), \\ v &= l\left(\frac{dv}{dz} + \frac{dw}{dy}\right). \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad (50)$$

\* We are here considering cases where, as in §§ 4, 5, the electric surface-forces may be neglected, being of the second order.



Substituting the values of  $dw/dx$ ,  $dw/dy$  from (48), we see that if we neglect  $lA$ ,  $lB$ ,  $lC$  in comparison with unity, we have

$$\left. \begin{aligned} u &= l \frac{du}{dz}, \\ v &= l \frac{dv}{dz}, \end{aligned} \right\} \dots \dots \dots (51)$$

Hence, if  $q$  denote the velocity parallel to a tangent-line at any point P of the wall, we have at P

$$q = l \frac{dq}{dn}; \dots \dots \dots (52)$$

or, if  $\lambda_1, \mu_1, \nu_1$  be the direction-cosines of the normal, and  $\lambda_2, \mu_2, \nu_2$  those of the tangent-line,

$$\lambda_2 u + \mu_2 v + \nu_2 w = l \left( \lambda_1 \frac{d}{dx} + \mu_1 \frac{d}{dy} + \nu_1 \frac{d}{dz} \right) (\lambda_2 u + \mu_2 v + \nu_2 w), \quad (53)$$

in which of course  $\lambda_2, \mu_2, \nu_2$  are to be treated as constants during the differentiations. Let us apply this to the case when P is any point  $(x, y)$  near the origin. The values of  $\lambda_1, \mu_1, \nu_1$  for this case have been given in (47), whilst we may write

$$\lambda_2 : \mu_2 : \nu_2 = dx : dy : (Ax + By)dx + (Bx + Cy)dy + \dots$$

Substituting in (53), and equating coefficients of  $dx, dy$ , we find

$$\left. \begin{aligned} u + (Ax + By + \dots)w &= l \left\{ \lambda_1 \frac{du}{dx} + \mu_1 \frac{du}{dy} + \nu_1 \frac{du}{dz} \right. \\ &\quad \left. + (Ax + By + \dots) \left( \lambda_1 \frac{dw}{dx} + \mu_1 \frac{dw}{dy} + \nu_1 \frac{dw}{dz} \right) \right\}, \\ v + (Bx + Cy + \dots)w &= l \left\{ \lambda_1 \frac{dv}{dx} + \mu_1 \frac{dv}{dy} + \nu_1 \frac{dv}{dz} \right. \\ &\quad \left. + (Bx + Cy + \dots) \left( \lambda_1 \frac{dw}{dx} + \mu_1 \frac{dw}{dy} + \nu_1 \frac{dw}{dz} \right) \right\}. \end{aligned} \right\} \dots (54)$$

In these equations  $u, v, w$ , &c., denote the values of these quantities at the point  $(x, y)$ , and must be expanded in terms of  $x, y$ . Performing the expansions, and equating coefficients of  $x, y$ , we get the following four relations:—

$$\left. \begin{aligned} \frac{du}{dx} &= l \left( -A \frac{du}{dx} - B \frac{du}{dy} + \frac{d^2u}{dx dz} + A \frac{dw}{dz} \right), \\ \frac{du}{dy} &= l \left( -B \frac{du}{dx} - C \frac{du}{dy} + \frac{d^2u}{dy dz} + B \frac{dw}{dz} \right), \\ \frac{dv}{dx} &= l \left( -A \frac{dv}{dx} - B \frac{dv}{dy} + \frac{d^2v}{dx dz} + B \frac{dw}{dz} \right), \\ \frac{dv}{dy} &= l \left( -B \frac{dv}{dx} - C \frac{dv}{dy} + \frac{d^2v}{dy dz} + C \frac{dw}{dz} \right). \end{aligned} \right\} \quad (55)$$

If we neglect  $lA$ ,  $lB$ ,  $lC$ , as before, these equations combined with the equation of continuity

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0$$

reduce to

$$\left. \begin{aligned} \frac{du}{dx} &= l \frac{d^2u}{dx dz}, \\ \frac{du}{dy} &= l \frac{d^2u}{dy dz}, \\ \frac{dv}{dx} &= l \frac{d^2v}{dx dz}, \\ \frac{dv}{dy} &= l \frac{d^2v}{dy dz}. \end{aligned} \right\} \quad (56)$$

If there is no slipping  $l=0$ , and the preceding equations then show that the following quantities all vanish at the origin—

$$\begin{aligned} u, \quad \frac{du}{dx}, \quad \frac{du}{dy}, \\ v, \quad \frac{dv}{dx}, \quad \frac{dv}{dy}, \\ w, \quad \frac{dw}{dx}, \quad \frac{dw}{dy}, \quad \frac{dw}{dz}, \quad \frac{d^2w}{dx^2}, \quad \frac{d^2w}{dx dy}, \quad \frac{d^2w}{dy^2}, \end{aligned}$$

the last three quantities vanishing in virtue of (49). We may therefore write in this case—

$$\nabla^2 w = \frac{d^2w}{dz^2},$$

a result which must also hold good approximately when  $l$  is not zero, provided it be small in comparison with the other linear magnitudes concerned.

IX. *Proceedings of Learned Societies.*

## GEOLOGICAL SOCIETY.

[Continued from vol. xxiv. p. 291.]

Nov. 9, 1887.—Prof. J. W. Judd, F.R.S., President, in the Chair.

THE following communications were read:—

1. "Note on the so-called 'Soapstone' of Fiji." By Henry B. Brady, F.R.S.

2. "On some Results of Pressure and of Intrusive Granite in Stratified Palæozoic Rocks near Morlaix, in Brittany." By Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., F.G.S.

The Author briefly described the banded Palæozoic slates in the neighbourhood of Morlaix, and gave a general account of their microscopic structure. They are greatly contorted and folded, and have evidently undergone very severe pressure. The result of this, as it appears to him, has been the development of minute scales of a light-coloured mica, especially in the darker (originally argillaceous bands) and certain corresponding changes in the more quartzose layers. The cleavage-planes often cut the surfaces of bedding and of this micro-foliation, which are parallel, at high angles, and so are of the nature of "*Ausweichungsschivage*."

In certain places these banded slates, after they have attained the aforesaid condition, have been affected by intrusive granites. The result has been the intensification of the changes which were already incipient. The quartz granules have been doubled in size, the flakes of mica have become four or five times as large, the black material of the argillaceous bands has been gathered into larger granules, and seemingly reduced in quantity (probably by partial oxidation of the carbon), and in some cases andalusite crystals or grains of considerable size have been developed. The rock has become comparatively hard, instead of friable, and the cleavage-planes are "soldered up" by the development of mica along them. In its general aspect one of these banded rocks (where free from andalusite) bears considerable resemblance, macroscopic and even microscopic, to one of the less coarsely crystalline, distinctly banded mica-schists, supposed by many to occupy a rather high position in the Archæan series.

3. "On the Position of the Obermittweida Conglomerate." By Prof. T. McK. Hughes, M.A., F.G.S.

The Author gave an account of a visit to the section at Obermittweida, 50 miles S.W. of Dresden, where there is an apparent intercalation of conglomerate and sandstone in a gneissic series. West of the stream at Obermittweida there is seen a crushed but not much altered conglomerate of felsite and other pebbles, above which gneiss and mica-schist rest, apparently in true sequence numerically. Below the conglomerate no rocks were seen, but at a little distance to the eastward coarse flaked muscovite-schists and gneissic rocks were exposed, apparently underlying it. By a diagram the Author showed how the conglomerate might belong to much newer beds caught in a synclinal fold of the schists, and he advanced various arguments in support of this explanation.

4. "On the Obermittweida Conglomerate: its Composition and Alteration." By Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., F.G.S.

The Author is indebted to Professor Hughes for the opportunity of examining a fine series of specimens of this rock, collected by the latter. The pebbles vary from well-rounded to subangular, some of the smaller fragments occasionally being practically unworn. The matrix is sometimes granular to the unaided eye, sometimes very fine. The whole mass has evidently been subjected to considerable pressure.

The associated gneiss is a moderately coarse gneiss, containing two micas and garnet. It has a general resemblance to rocks which, in the Alps, appear to occur about or rather below the middle of the crystalline series.

The fragments consist of various kinds of rocks. Those examined microscopically are referred to granitoid rock (3 varieties), mica-schist, quartz-schist, quartzite, hälleflinta (?).

The matrix is almost wholly composed of quartz and mica (two species, but mostly brown) with some felspar. The materials appear to have undergone a certain amount of metamorphism, by augmentation of the original fragments, and to some extent by development of new minerals. The author is of opinion that the materials are rather more altered than is usual in Palæozoic greywackes and conglomerates, but that the comparatively small amount of alteration, and the character of the included fragments, render it highly improbable that the conglomerate is in stratigraphical sequence with the above-described gneiss, or with any similar series of rocks; and so, if Archæan, it must belong to one of the latest epochs in that period.

5. "Notes on a part of the Huronian Series in the neighbourhood of Sudbury (Canada)." By Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., F.G.S.

The specimens noticed by the Author were in part collected by him in the summer of 1884, when the Canada Pacific Railway was in process of construction, and in part subsequently supplied to him by the kindness of Dr. Selwyn, Director-General of the Geological Survey of Canada.

The eastern edge of the district assigned to the Huronian consists of rocks, which may possibly be part of the Laurentian series modified by pressure. But after crossing a belt of these, barely a mile wide, there is no further room for doubt. All the rocks for many miles are distinctly fragmental, except certain intrusive diabases or diorites. These fragmental rocks are grits, conglomerates and breccias, which are described as far as about 2 miles west of Sudbury. The included fragments in these rocks appear to have undergone some alterations subsequent to consolidation: these are described. In some cases the changes appear to be anterior to the formation of the fragments. The matrix also has undergone some change, chiefly the enlargement of quartz grains, and the development or completion of mica-flakes, as in the Obermittweida rock.

The Author gave some notes on other specimens collected by him



along the railway, further west, and on those supplied to him from near Lake Huron by Dr. Selwyn. As a rule these are but little altered. Some contain fragments of igneous rocks, apparently lavas.

The author discusses the significance of the changes in these rocks, as bearing on general questions of metamorphism, and states that, in his opinion, the name Huronian, at present, includes either a series of such great thickness that the lower beds are more highly altered than the higher, or else two distinct series; and he inclines to the latter view. Both, however, must be separated from the Laurentian by a great interval of time, and neither exhibits metamorphism comparable with that of a series of schists and gneisses, like the so-called Montalban. The newer reminds him often of the English Pebidians.

December 7.—Prof. J. W. Judd, F.R.S., President, in the Chair.

The following communications were read:—

1. "A Letter from H.M. Secretary of State for the Colonies, enclosing an account of recent Discoveries of Gold in the Transvaal."

The deposits in which gold has been found, locally known as "banket," consist of a quartz-conglomerate forming so-called "reefs," which traverse the veldt parallel to, but at a short distance from, the rocky ridge of Witwatersrand. These masses always dip to the south, but at angles varying from  $30^{\circ}$  up to  $90^{\circ}$ . The "reefs" are believed to have been discovered by Mr. Struben, an English gentleman long resident in the country. The "main reef" has been traced for twenty-five or thirty miles, and varies in breadth from 3 feet 6 inches to 15 feet; parallel and branching "reefs" of smaller dimensions have also been found. The yield of gold is said to be very variable in different portions of the "reef," different samples with from 3 oz. to  $\frac{1}{2}$  oz. per ton occurring in close proximity. So far as observation has gone (and the deepest workings have only reached a depth of from 70 to 150 feet), the yield of gold has generally increased as the reefs are followed downwards.

2. "On the Age of the Altered Limestone of Strath, Skye." By Dr. Archibald Geikie, F.R.S., V.P.G.S.

The remarkable alteration of the limestone of Strath into a white saccharoid marble, first described by Macculloch, has hitherto been regarded as an instance of contact-metamorphism in a rock of Liassic age. The various writers who have described the geology of the district have followed Macculloch in classing the whole of the ordinary and altered limestone with the Secondary series of the Inner Hebrides. The author, however, saw reason in 1861 to suspect that some part of the limestone must be of the age of the Durness Limestone of Sutherland, that is, Lower Silurian; and he expressed this suspicion in a joint paper by the late Sir R. I. Murchison and himself, published in the 18th volume of the Quarterly Journal of the Society. He has recently returned to the subject,

and now offers lithological, stratigraphical, and palæontological evidence that the altered limestone is not Lias but Lower Silurian.

In lithological characters the limestone, where not immediately affected by the intrusion of the eruptive rocks, closely resembles the well-known limestones of the west of Sutherland and Rosshire. It is not more altered than Palæozoic limestones usually are. It contains abundant black chert-concretions and nodules, which project from the weathered surfaces of the rock exactly as they do at Durness. These cherts do not occur in any of the undoubted Lias limestones of the shore-sections. The limestone lies in beds, which, however, are not nearly so distinct as those of the Lias, and have none of the interstratifications of dark sandy shale so conspicuous in the true Liassic series.

The stratigraphy of the altered limestone likewise marks it off from the Lias. There appears to be a lower group of dark limestones full of black cherts, and a higher group of white limestones with little or no chert, which may be compared with the two lower groups of the Durness Limestone. A further point of connexion between the rocks of the two localities is the occurrence of white quartzite in association with the limestone at several places in Strath, and of representatives of the well-known "fucoid beds" at Ord, in Sleat. These latter strata form a persistent band between the base of the limestone and the top of the quartzite, which may be traced all the way from the extreme north of Sutherland southward into Skye.

Palæontological evidence confirms and completes the proof that the limestone is of Lower Silurian age. The Author has obtained from the limestone of Ben Suardal, near Broadford, a number of fossils which are specifically identical with those in the Durness Limestone, and so closely resemble them in lithological aspect that the whole might be believed to have come from the same crag. Among the fossils are species of *Cyclonema*, *Murchisonia*, *Maclurea*, *Orthoceras*, and *Piloceras*.

The relations of the limestones containing these fossils to the other rocks were traced by the Author. He showed that the Lias rests upon the Silurian limestone with a strong unconformability, and contains at its base a coarse breccia or conglomerate, chiefly composed of pieces of Silurian limestone, with fragments of chert and quartzite. The metamorphism for which Strath has been so long noted is confined to the Silurian limestone, and has been produced by the intrusion of large bosses of granophyre (Macculloch's "syenite") belonging to the younger, or Tertiary series of igneous rocks.

3. "On the Discovery of Trilobites in the Upper Green (Cambrian) Slates of the Penrhyn Quarry, Bethesda, near Bangor, North Wales." By Henry Woodward, F.R.S., V.P.G.S.

4. "On *Thecospondylus Daviesi*, Seeley, with some Remarks on the Classification of the Dinosauria." By Prof. H. G. Seeley, F.R.S., F.G.S.

X. *Intelligence and Miscellaneous Articles.*

## ON ELECTRICITY PRODUCED BY THE FRICTION OF DROPS.

BY J. ELSTER AND H. GEITEL.

THE authors sum up the results to which their investigation leads as follows:—

(1) If the current of a spray-producer is directed against a solid wetted with water, any electrification which may be observed must not necessarily be attributed to friction against the surface. In this mode of arranging the experiment, the inductive action of even very low tensions in the air exerts a preponderating influence. Even where the droplets are leaving the mouth of the spray-producer in the unelectrified condition, the plate which receives the droplets must be charged by induction to the potential of the neighbourhood by the droplets.

It follows from this that experiments on the excitation of electricity by the friction of drops must be received with the greatest reserve, if suitable measures are not taken to eliminate the electromotive forces produced by induction, or to make an allowance for the disturbance which they produce, a problem the complete solution of which has hitherto failed us.

(2) On some bodies on which there is no wetting, the excitation of electricity by friction preponderates so considerably over that by induction that the influence of the latter may in most cases be disregarded.

To these bodies belong those whose surface is coated with wax, shellac, sulphur, or fat. The leaves of certain plants were found to be particularly active.

A jet of water which disperses in drops acts like a spray-producer.

(3) If metal plates are coated with liquids which do not moisten, a powerful electrification may be imparted to them by causing an *extremely fine* jet of water to glide over them in such a way that the place of dispersion is over or on the plate. For the success of this experiment it is necessary that the electrical layer should be a good insulator. If this is not the case, as, for instance, with the leaves of plants, electrification is only found in the discontinuous part of the jet. In all the cases mentioned under (2) and (3) the rubbed body is negative, and therefore the water is positive.

(4) Bodies of such high temperature that Leidenfrost's drops form on them may also be electrified by friction. All the circumstances which favour the occurrence of the spheroidal condition of a liquid, also produce an increase of the electromotive force at the place at which the solid and liquid surfaces are in contact. In this the electrification of hot water is negative; when the spheroidal condition ceases it is positive, and below  $110^{\circ}$  to  $120^{\circ}$  there is no appreciable disengagement of electricity.

The deportment of ether is analogous; at the ordinary temperature it electrifies the rubbed body positively, and at higher temperatures negatively.

With alcohol there is no such change in the sign of the charge.—*Wiedemann's Annalen*, August 1887.

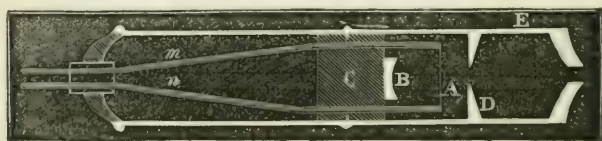
#### A NEW INSTRUMENT FOR THE MEASUREMENT OF RADIATION.

BY C. C. HUTCHINS.

The difficulties which attend the use of the thermopile as an accurate measurer of radiations are familiar to all who have had any experience with that instrument. The slowness of its indications, and the long time required for it to return to zero, are defects which entirely unfit it for many delicate experiments.

It occurred to the writer that sensitiveness to radiation might as well be secured by employing a very thin thermal junction with some condensing arrangement, as by the use of several pairs of stout bars as in the ordinary thermopile; for the thin junction would be heated to a much higher temperature than a thick one by a given quantity of heat, and have the great advantage of quickly parting with its heat and returning to the temperature of the surrounding atmosphere.

The instrument is constructed upon these principles as follows:—A tube of vulcanite ten inches long, two and a half inches in diameter, is stopped near the middle by a plug of wood. The tube is made separable, and this plug serves to unite its two halves as well as to support the working parts. Through the plug pass two small copper rods projecting about an inch above the plug towards the front of the instrument, and passing out through its back, where they serve to attach wires extending to a galvanometer.



E, tube of vulcanite; C, plug of wood; *m*, *n*, copper rods; A, thermal junction; B, concave mirror; D, stop.

The thermal junction is made by uniting with hard solder a bit of watch-spring and a bit of flattened copper wire. The whole is then worked to a ribbon 1 millim. wide, .03 millim. thick and 25 millim. long. The two ends of this ribbon are then soldered to the two copper rods so that the junction may be midway between them.

A concave mirror of glass, silvered upon first surface, is so secured upon the plug that the junction is exactly at its focus. The front of the tube is provided with an opening of any convenient size, and stops to limit the diameter of the entering ray.

The accompanying sketch will make the details clear. Its working has been very satisfactory. It requires no longer to return to zero than for the galvanometer-needle to come to rest, and is correspondingly rapid and dead-beat in its action. It is much more sensitive than a thermopile of the same exposed area.



An instrument in actual use having an opening of 8 millin. deflects its galvanometer 30 divisions of its scale when the hand is held a foot from the opening. A lighted match at six feet drives the needle around to its top.—Silliman's *American Journal*, December 1887.

APPARATUS FOR ILLUSTRATING THE FALL OF BODIES IN A  
VACUUM. BY J. PULUJ.

This apparatus has the advantage over others that it demonstrates the property that all bodies fall at the same rate *in vacuo* in a very simple manner and always with certainty. It consists of a glass tube 4 centim. wide, and 150 centim. in length, closed at both ends, having been previously exhausted; in it are contained the bodies, an iron bullet 1.5 centim. in diameter, and a light feather, in the quill-end of which is a fine iron needle-point 2 millim. in length. At the bottom of the vertical tube a caoutchouc stopper is inserted to protect the glass tube from breakage by the falling bullet; and at the top a brass flanged tubulure is cemented, in which an electromagnet with an iron core can be screwed. The tube is suspended vertically in the fork of a wooden support, by means of two pegs affixed to the electromagnet. For exciting the electromagnet a powerful battery of about 3 Bunsen's elements, or an accumulator consisting of three coils, is used.

To make the experiment, the tube is gently inclined until the feather and the ball are in the field of the electromagnet, the electrical current is closed, and the tube placed on the support. Both the feather and the ball are held at the top of the glass tube, and fall simultaneously the moment the current is opened.—*Berichte der Kaiserlich. Akad. in Wien*, Nov. 3, 1887.

ATTEMPT TO APPLY THE DIFFUSION OF GASES AND VAPOURS  
THROUGH POROUS BODIES TO DETERMINING THE AMOUNT OF  
MOISTURE AND CARBONIC ACID IN THE SURROUNDING AIR.  
BY F. SCHIDLOWSKY.

The author has repeated the experiments of Dufour on the diffusion of air and aqueous vapour, and has come to the conclusion that in this phenomenon the absorption of vapour by porous bodies plays an important part.

If, for instance, we take a closed porous cylinder which contains dry air under ordinary atmospheric pressure, and in connexion with a water-manometer, and we place it in another cylinder in which the air is always saturated with aqueous vapour by the presence of water, then, according to Dufour, the manometer at once shows a decrease of pressure of about 10 to 12 millim., which afterwards becomes equalized.

Conversely, when the same cylinder is filled with moist air, and brought inside a dry cylinder, there is an increase of about 20 millim., which also is afterwards equalized.

These phenomena are thus explained by the author:—

Between both sides of the inner porous cylinder there is obviously a difference of pressure equal to the pressure of vapour at the temperature of the experiment. The air goes from the interior of the body (in the first experiment) towards the outside, and the vapour in the opposite direction; but the latter is absorbed by the porous sides, and we have first of all a decrease of pressure, which rapidly vanishes, since the vapour diffuses, according to Graham's law, more rapidly than dry air. The author explains in an analogous manner the converse experiment of Dufour. Similar experiments of his own, which essentially agree with those of Dufour, have confirmed this explanation.

The author applies the diffusion of gases and vapours through porous bodies to the construction of a hygrometer, or gas-indicator. The hygrometer in question consists of a vessel (cylinder) which is filled with water, connected with a water-manometer, and covered by a porous plate. If this apparatus is brought into free air, the manometer soon rises, and the higher, the further the surrounding air is from the point of saturation with aqueous vapour.

The manometer falls when the vessel is filled not with water but with sulphuric acid, and thus dry air is present. The change of pressure is a function of the temperature, and is different for different porous plates. The apparatus can be easily graduated.—*Beiblätter der Physik*, No. 9, 1887 (from *Journal of the Russian Physical and Chemical Society* for 1886).

## ON ELECTRICAL TRANSPORTATION IN LIQUIDS.

BY S. TERESCHIN.

The transport of liquids in capillary tubes by constant electrical currents has hitherto been investigated for comparatively small electrical forces.

At the suggestion of Prof. Quinke, instead of a constant voltaic current, I used the electrical current which flows to earth from the inner coating of a Leyden jar through the column of liquid, and through a sensitive reflecting-galvanometer. The outer coating of the Leyden jar of flint glass was put to earth. The inner coating was kept at constant potential by rotating a Holtz's machine; and this electrical potential  $P$  was measured by a Righi's electrometer with bifilar suspension, in electrostatic units of the C.G.S. system. By multiplying these numbers by 300 we get the electrical potential expressed in volts.

In these observations I used an apparatus of the same construction as that used by Prof. Quinke; it had a flint glass tube 0.738 millim. in diameter, which was inclined at an angle  $\phi$  with the horizon. The distance  $a$  of the electrodes was 289 millim. The third platinum wire interposed between the two former, made it possible to diminish to one half the length of column of the liquid traversed by the current.

The results collated in the following tables give the observations for water, methylic and ethylic alcohols. The first column gives

the difference of potential  $P$  in C.G.S. units; the second the displacement of the meniscus of the liquid; the third the current in amperes. The last column contains the constant called  $b$  by Prof. Quincke, calculated from the equation:

$$b = \frac{1.9}{300} \frac{r^2 v \sin \phi}{P}.$$

The electromotive force of a Grove's element being put = 1.9 volt.

P in C.G.S. units.	Length $a=289$ millim.		$b$ .
	$v$ in millim.	J. $10^8$ amp.	
Water ( $\phi=2^\circ 52'$ ). Entire length.			
17.06	19	163	0.0000480
24.07	31	232	0.0000555
29.54	38	299	0.0000555
34.12	43	333	0.0000543
38.15	48	348	0.0000543
Mean .....			0.0000535
Water. Half length.			
17.06	21	.....	0.0000530
24.07	31	.....	0.0000553
29.54	36	.....	0.0000525
34.12	42	.....	0.0000540
Mean .....			0.0000537
Methyl Alcohol ( $\phi=1^\circ 9'$ ). Entire length.			
17.06	22	129	0.0000223
24.07	32	171	0.0000230
29.54	47	199	0.0000275
34.12	50	224	0.0000253
Mean .....			0.0000245
Ethylic Alcohol ( $\phi=1^\circ 9'$ ). Entire length.			
17.06	15	34	0.0000152
24.07	22.5	45	0.0000161
29.54	29.5	55	0.0000168
34.12	36	67	0.0000162
38.15	37	70	0.0000167
Mean .....			0.0000162

The height of ascent was thus found to be proportional to the

differences of potential. It was the same, whether the current went through the whole, or half the length of the tube. The constant  $b$  was found by Prof. Quinke for ordinary German glass to be almost equal:

For water ..... 0.000055 to 0.000067

For ethylic alcohol ..... 0.000034

The resistance of the liquids altered considerably during the experiments. In the mean it was as follows:—

	Resistance of the entire column of liquid.	Specific resistance, Hg=1.
Water .....	$319 \cdot 10^7$ ohms	$4722 \cdot 10^6$
Methyl alcohol .....	$430 \cdot 10^7$ ohms	$6365 \cdot 10^6$
Ethylic alcohol .....	$1570 \cdot 10^7$ ohms	$23350 \cdot 10^6$

If greater forces were used, water showed a sudden change of resistance as soon as the electrical force  $P/a$  in the liquid exceeded a certain limiting value; the deflection of the galvanometer-needle exhibited a sudden fall of potential. This sudden fall is analogous to that which is observed in dielectric liquids, and is explained by the sudden occurrence of an electrolytic decomposition\*. The limiting value of  $P/a$  for water is considerably less than for good insulating liquid dielectrics. As a rough approximation we may put for it the value 1.32 to 1.45 C.G.S. in the case of the whole length, and 1.61 to 1.98 C.G.S. in the case of half the length.

I found no alteration with alcohol.

Observations on other dielectrics, as bisulphide of carbon, ether, oil of turpentine, and rape-oil, all showed a considerable transport in the direction of the positive current. I could not, however, obtain constant numbers.—Wiedemann's *Annalen*, No. 10, 1887.

#### ELECTRICAL IMPRESSIONS. BY K. TSCHECOWITSCH.

If a glass plate is laid on tinfoil, and on the glass a coin, and if the two metals are connected with the two conductors of an electrical machine, then, as Karsten has shown, after a greater or less number of turns of the machine, the coin can be removed, and by breathing on the glass, an impression is produced of the coin which laid on the glass.

The author has observed that such impressions may be easily fixed on glass, if, instead of breathing on the glass, it be coated with a dilute solution of stearine, yellow oxide of mercury, oxide of zinc, or the like in benzol.

If the glass is covered with a thin layer of fat (vaseline is best) the coin leaves directly on the glass after electrification a perfectly distinct impression.

The figures differ with the kind of electricity with which the coin is charged.—*Beiblätter der Physik*, No. 10, 1887 (from the *Journal of the Russian Physical and Chemical Society*, 1887).

\* Quinke, Wied. *Ann.* xxviii. p. 547 (1886).



THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

---

[FIFTH SERIES.]

---

FEBRUARY 1888.

---

XI. *On some Questions in the Kinetic Theory of Gases.*  
By Prof. LUDWIG BOLTZMANN\*.

§ 1. *On Avogadro's Law and the Coefficient of Friction.*

IN my former papers on the heat-equilibrium of a mixture of two gases, I have nowhere made mention of the physically less important case when the molecules of one of the mixed gases come into collision among themselves comparatively seldom, or when the number ( $N_1$ ) of molecules in the unit volume is greatly smaller for the one gas than the number ( $N_2$ ) for the other.

In reference to this, Prof. Tait, in his first paper on this subject in the Transactions of the Royal Society of Edinburgh†, on page 78, reckons the following amongst the necessary assumptions for the proof of Boyle's law:—"That there is perfectly free access for collision between each pair of particles, whether of the same or of different systems; and that, in the mixture the number of particles of one kind is not overwhelmingly greater than that of the other kind;" and adds to it the remark:—"This is one of the essential points which seem to be wholly ignored by Boltzmann and his commentators. There is no proof given by them that one system, while regulating by its internal collisions the distribution of energy among its own members, can also by impacts regulate the distribution of energy among the members of another

\* Translated, at the request of the Author, from advance proof-sheets from the *Sitz. der Kais. Akad. der Wissensch. Wien*, Bd. xcvi.

† Tait, *Phil. Trans. Edinb.* vol. xxxiii. part i. p. 65 (1886); in abstract in *Phil. Mag.* vol. xxi. (1886) p. 343.

system, when these are not free to collide with one another.—In fact, if (to take an extreme case) the particles of one system were so small, in comparison with the average distance between any two contiguous ones, that they practically had no mutual collisions, they would behave towards the particles of another system much as Le Sage supposed his ultra-mundane corpuscles to behave towards particles of gross matter &c.”

Against this reproach I endeavoured to defend both myself and those who in their later investigations have followed a similar line of thought (who are probably intended by Prof. Tait's word “commentators”) in the paper “On the Assumptions necessary for the Theoretical Proof of Avogadro's Law,”\* where on page 629 I have proved† the following general equation:—

$$\frac{2}{\pi} \frac{dE}{dt} = \int_0^\infty \int_0^\infty \int_0^\pi \int_0^{2\pi} v^2 V^2 r \tau s \sigma \, dv \, dV \, dT \, dS \, dO \\ \times \left[ \lambda^2 (f'f_1' - f_1f) l \frac{f_1'}{f_1} + \Lambda^2 (F'F_1' - FF_1) l \frac{F_1'}{F_1} \right. \\ \left. + 2\delta^2 (f'F_1' - fF_1) l \frac{f_1'}{F_1'} \right].$$

In obtaining this equation I have generally made precisely the same assumptions as Professor Tait, except that I have not made the least assumption with reference to the relative magnitude of the diameters  $\lambda$  and  $\Lambda$  of the molecules of the two gases, nor as to the magnitude of the ratio  $N_1 : N_2$ . For the stationary condition  $dE : dt$  must vanish; whence it follows, as I have repeatedly shown, that:—

- (1) Each of the gases assumes Maxwell's distribution of velocities (or, as Prof. Tait says, the special condition);
- (2) The mean kinetic energy of a molecule is the same for both gases.

Simply to show why, in my former papers, I have not specially mentioned the cases where  $\lambda$ ,  $\Lambda$  or  $N_1$ ,  $N_2$  have very small or very large values, I remark that these propositions

\* L. Boltzmann, *Wien. Sitzb.* Bd. xciv. p. 613 (1886); *Phil. Mag.* [5] vol. xxiii. p. 305 (1887).

† The calculations here referred to, as well as those upon the Heat-equilibrium of Heavy Gases (*cf.* § 4 of the text), may be considerably simplified by the use of Lorentz's method, as fully explained in that paragraph. I am indebted to Mr. Burbury for kindly calling my attention to the possibility of further simplifications, especially by use of the extremely useful proposition, “that for the relative velocity after impact with given magnitude and direction of velocities before impact, but with optional line of centres, every direction is equally probable,” and I shall probably return to all these points on some subsequent occasion.

hold good for any values whatever of  $\lambda$ ,  $\Delta$ , and  $\delta$ ; consequently also when  $\lambda=0$ , and only  $\Delta$  and  $\delta$  differ from zero, and even if  $\lambda=\Delta=0$ , and only  $\delta$  differs from zero, which amounts to the assumption that neither the molecules of the first kind nor those of the second kind enter into collisions amongst themselves, but only the molecules of the first kind enter into collision with those of the second kind. It is quite unintelligible to me how Prof. Tait can represent\* the matter as if this latter assumption were a fundamental assumption, which I had excluded from my investigations, so that my results would only hold good under this latter assumption.

If the molecule-diameter is very much less for the one kind of gas than for the other, so that, for example,  $\lambda$  is nearly zero and  $\Delta$  nearly equal to  $2\delta$ , we have the case considered by Prof. Tait in the passage quoted, that the molecules of the first kind of gas enter into collision amongst themselves very seldom, but, on the contrary, very frequently with those of the other kind. It follows at once from the above formula, that then also the molecules of the first kind of gas are brought into the special condition only by collision with those of the other kind of gas, and the mean kinetic energy of a molecule must be the same for both gases, and moreover very quickly, if only the impacts of molecules of the first kind with those of the second kind occur very frequently†. This case is the clearest example that the special condition and Avogadro's law may both hold under circumstances which Prof. Tait expressly excludes from their action.

Also the second just-mentioned paper of Prof. Tait contains not the least proof that in this simplest case Avogadro's law ceases to hold or does not hold under the special condition of one of the gases, or that in any other respect the objection contained in the quotation given above is justified. For, from the circumstance that in this case Prof. Tait's proof of Avogadro's law is no longer applicable, it does not obviously follow that the law itself ceases to hold good. Prof. Tait's second paper contains, on the contrary, simply general statements, supported by English and Greek quotations, that my assumptions are not allowable. But how, if these assumptions are not allowable, can I be reproached with having ignored them in my former

\* Phil. Trans. Edinb. vol. xxxiii. part ii. p. 251 (1887); in abstract, Phil. Mag. [5] vol. xxiii. pp. 141 & 433.

† Only if the molecules of a certain kind enter into collision both with each other and with the rest only very seldom, then, as a matter of course, the special condition will also be reached by these very late. But not to have mentioned this cannot be made a reproach either to me or any one of my commentators, since there was not the least occasion to do so.

paper? Again, how can Prof. Tait say, on page 255, "I have not yet seen any attempt to prove that two sets of particles which have no internal collisions tend to the state assumed by Prof. Boltzmann," after he has not said one word in actual refutation of my proof, and in the *Philosophical Magazine* even admits its correctness, no doubt without having read it. But this seems to be the usual habit of my illustrious critic, not to read the works of those authors upon which he sits in judgment. Only in this way can we explain why he has given (on page 260 of his second paper) as new the same formula for the coefficient of friction which I have obtained essentially in the same way as Prof. Tait, in my treatise "On the Theory of Gaseous Friction," part ii.\*, and have given as formula (9), page 45; the coefficient of B in the expression which Prof. Tait gives in line 14 of the page quoted is the coefficient of friction. If in this coefficient we substitute for  $C_1$  the value given by Prof. Tait on page 257 and replace the letters P and  $s$  by  $m$  and  $\delta$  respectively, we obtain precisely my formula (9). Of course, my formula (8) on page 44 also agrees exactly with the value found by Prof. Tait on page 73 of his first paper for  $ev$ , which moreover O. E. Meyer had already calculated before me.

If Prof. Tait had read my paper he might also have spared himself the trouble of the numerical evaluation of the definite integral, since I have already given (in the formula (9) quoted) the numerical value  $C_1 : 3\pi = 0.0889426$ ; whence by multiplication by  $3\pi = 9.42478$  we obtain exactly Tait's value  $C_1 = 0.838$ , only that I have attempted greater accuracy.

If, in the same place, I have not treated diffusion and conduction of heat according to the same method which Prof. Tait has adopted, this is simply partly because the calculations in question can be easily effected without this in the same form; partly because, as I have shown both in the passage referred to and in the first part of my "Theory of Gaseous Friction," † at the beginning of the first section, this method does not yield that which at first sight it seems to yield. In this expression (first calculated by me) for the coefficient of friction, exactly as in the older expressions of Maxwell, terms of the same order of magnitude as those giving the result are neglected, and it is therefore not possible to tell whether one comes closer to the truth than the other.

This also appears in the great uncertainty of the results to which this method leads. E. Meyer, in his book 'The Kinetic Theory of Gases' (Breslau, 1877), has already followed a line of thought essentially similar, although somewhat more tedious,

\* Boltzmann, *Wien. Sitzber.* vol. lxxxiv. p. 40 (1881).

† *Ibid.* vol. lxxxi. p. 117 (1880).



but fundamentally equally sound, and obtained (on page 320) a different numerical result.

If we wish to neglect as few terms as possible of the order of magnitude of the coefficient of friction, we must improve the method adopted by Tait and myself, which in fact leads to Meyer's numerical result. We must in fact observe that each molecule in its path passes through layers in which a progressively different motion of masses prevails, and that consequently the probability of collision has a different value in each layer. If we adopt Prof. Tait's notation exactly, then, taking this circumstance into account, his conclusion must be modified somewhat as follows. As on page 259 of his second paper, let the layer of gas parallel to the  $yz$ -plane which has the abscissa  $x$  move with the mean velocity  $Bx$  parallel to the  $Y$ -axis. If in the unit volume there are, on the whole,  $n$  molecules, then, according to Prof. Tait, there are

$$n \cdot v \cdot e \cdot v \cdot dx$$

molecules emitted in unit time by the unit surface of the layer lying between  $x$  and  $x + dx$  whose velocities lie between  $v$  and  $v + dv$ . We will denote the product  $ev$  by  $f(v)$ . If, further, we put

$$v = \sqrt{\frac{h^3}{\pi^3}} e^{-hp(p^2+q^2+r^2)} dp dq dr,$$

we obtain

$$N_0 = n dp dq dr dx \sqrt{\frac{h^3}{n^3}} e^{-h(p^2+q^2+r^2)} f(\sqrt{p^2+q^2+r^2})$$

for the number of molecules emitted by the layer whose velocity relative to the resultant motion of the layer has components along the three axes of coordinates which respectively lie between the limits  $p$  and  $p + dp$ ,  $q$  and  $q + dq$ ,  $r$  and  $r + dr$ . But since the layer itself has the velocity  $Bx$  in the direction of the  $Y$ -axis, the components of the absolute velocity of these emitted molecules lie between the limits  $p$  and  $p + dp$ ,  $q + Bx$  and  $q + Bx + dq$ , and  $r$  and  $r + dr$ . Of these  $N_0$  molecules let  $N$ , without further coming into collision, reach the plane of abscissa  $\xi$ . Let us further imagine the layer which lies between  $\xi$  and  $\xi + d\xi$  constructed, and ask how many of our  $N$  molecules will come into collision in this layer. As Prof. Tait finds in his first paper (p. 73), in a layer whose mean velocity is zero, during the time  $dt$  out of  $N$  molecules, which all move with the same velocity  $v$ ,  $N \cdot e \cdot v \cdot dt = N dt \cdot f(v)$  molecules come into collision.

In the above case the  $N$  molecules have velocity-components  $p$ ,  $q + Bx$ ,  $r$  in the directions of the axes of coordinates, but the layer itself has the mean velocity  $B\xi$  in the direction of the  $Y$ -axis; the relative motion is therefore exactly as if the

layer itself were at rest, but the  $N$  molecules had the velocity

$$v = \sqrt{p^2 + (q + Bx - B\xi)^2 + r^2}.$$

The time  $dt$  which each of the  $N$  molecules requires to pass through the layer is  $d\xi : p$ . Since the number of impacts is evidently dependent simply upon the relative motion, the number of those of our  $N$  molecules which in the layer  $d\xi$  come into collision is

$$N / (\sqrt{p^2 + (q + Bx - B\xi)^2 + r^2}) \cdot \frac{d\xi}{p}.$$

This expression also gives the decrease of the number  $N$  in the layer  $d\xi$ , and may therefore be denoted by  $-dN$ . If the total velocity of the layers is very small in comparison with the velocity of the molecular motion,  $B$  may be taken to be very small, and the function  $f$  may be determined according to Taylor's theorem. If, further, we put  $f$  for  $f(\sqrt{p^2 + q^2 + r^2})$  and  $f'$  for  $f'(\sqrt{p^2 + q^2 + r^2})$ , where the dash denotes the derived function, we have

$$-dN = N \left( f + qBf' \frac{x - \xi}{\sqrt{p^2 + q^2 + r^2}} \right) \frac{d\xi}{p},$$

$$N = N_0 e^{-\frac{\xi f}{p} - \frac{qB\xi(2x - \xi)f'}{2p\sqrt{p^2 + q^2 + r^2}}};$$

or, since  $B$  may be assumed to be very small,

$$N = N_0 \cdot \left[ 1 - \frac{qB\xi(2x - \xi)f'}{2p\sqrt{p^2 + q^2 + r^2}} \right] \cdot e^{-\frac{\xi f}{p}}.$$

If in this expression we put  $\xi = x$ , we obtain the number of those of our  $N_0$  molecules which reach the  $YZ$ -coordinate plane without coming into collision. Since each of these molecules has the velocity-component  $q + Bx$  in the direction of the  $Y$ -axis, we find the total momentum estimated with reference to the  $Y$ -axis carried through the unit surface of the  $YZ$ -plane in unit time from right to left by multiplying by  $P(q + Bx)$  and integrating with reference to  $x$  from 0 to  $\infty$ , with reference to  $p$  from  $-\infty$  to 0, but with reference to  $q$  and  $r$  from  $-\infty$  to  $+\infty$ . Here  $P$  is the mass of a molecule. Consequently, again neglecting  $B^2$ ,

$$M = P \cdot n \sqrt{\frac{h^3}{\pi^3}} \int_{-\infty}^{+\infty} dq \int_{-\infty}^{+\infty} dr \int_{-\infty}^0 dp \int_0^{\infty} f dx \\ \left( q + Bx - \frac{Bx^2 q^2 f'}{2p\sqrt{p^2 + q^2 + r^2}} \right) \cdot e^{-h(p^2 + q^2 + r^2)} \cdot e^{-\frac{x f}{p}}.$$

Already the perfect symmetry with reference to the YZ-plane shows that the equal oppositely-directed momentum from left to right is carried through the YZ-plane, and that therefore the coefficient of friction is

$$\eta = 2M : B.$$

The integrations are effected by putting

$$p = -v \cos \theta, \quad q = v \sin \theta \cos \lambda, \quad r = v \sin \theta \sin \lambda.$$

Integrating first with reference to  $\lambda$  from 0 to  $2\pi$ , then with reference to  $x$  it follows that

$$\eta = 2Pn \sqrt{\frac{\bar{h}^3}{\pi}} \int_0^\infty v^4 e^{-hv^2} dv \int_0^{\frac{\pi}{2}} d\theta \sin \theta \left( \frac{2 \cos^2 \theta}{f} - \frac{v \cos^2 \theta \sin^2 \theta f'}{f^2} \right).$$

Integration with reference to  $\theta$  gives

$$\eta = \frac{4}{3} Pn \sqrt{\frac{\bar{h}^3}{\pi}} \int_0^\infty v^4 e^{-hv^2} dv \left( \frac{1}{f} - \frac{vf'}{5f^2} \right),$$

and the partial integration of the negative term

$$\eta = \frac{8}{15} Pn \sqrt{\frac{\bar{h}^3}{\pi}} \int_0^\infty \frac{v^6}{f} e^{-hv^2} dv.$$

If we observe that, if  $s$  denotes the diameter of a molecule,

$$f = ev = \pi n s^2 \sqrt{\frac{\bar{h}^3}{\pi}} \left[ \frac{1}{\bar{h}^2} e^{-hv^2} + \left( \frac{1}{\bar{h}^2 v} + \frac{2v}{\bar{h}} \right) \int_0^v e^{-hv^2} dv \right],$$

it follows that

$$\eta = \frac{8P}{15\pi s^2 \sqrt{\bar{h}}} \int_0^\infty \frac{z^7 e^{-z^2} dz}{ze^{-z^2} + (2z^2 + 1) \int_0^z e^{-z^2} dz}.$$

This is exactly the expression found by O. E. Meyer. I will not assert that it is really more accurate than that calculated by Prof. Tait and myself, since by other methods one would obtain other numerical results; but it is at least of equal authority. This value is smaller than that calculated by Maxwell for the coefficient of friction by about one twentieth, whilst the value calculated by Prof. Tait is nearly as much greater.

The remarks here made at length with reference to gaseous friction of course apply equally to Prof. Tait's method of calculating diffusion and conduction of heat.

Two other points connected with the theory of gaseous friction may be mentioned here:—

(1) If a gas move as a whole, with a constant very small

velocity  $b$ , in the direction of the  $y$ -axis, then the number of molecules whose absolute velocity lies between  $v$  and  $v+dv$ , and makes with the positive  $y$ -axis an angle lying between  $\epsilon$  and  $\epsilon+d\epsilon$ , is equal to

$$2n\sqrt{\frac{h^3}{\pi}} e^{-hv^2-hb^2+2hbv\cos\epsilon} v^2 \sin\epsilon d\epsilon dv.$$

If we consider all the molecules having a given absolute velocity  $v$ , we find for their mean velocity parallel to the  $y$ -axis the value

$$\frac{v \int_0^\pi e^{2hbv\cos\epsilon} \cos\epsilon \sin\epsilon d\epsilon}{\int_0^\pi e^{2hbv\cos\epsilon} \sin\epsilon d\epsilon} = v \left( \frac{e^{4hbv} + 1}{e^{4hbv} - 1} - \frac{1}{2hbv} \right),$$

which, for small values of  $b$ , reduces to  $\frac{2hbv^2}{3}$ , which latter value is of course more easily obtained by assuming  $b$  very small at once. The mean velocity parallel to the  $y$ -axis is therefore not perhaps equal to  $b$  for all molecules, but greater for the more rapid molecules. If  $b$  is not constant, but equal to  $Bx$ , as is the case in gaseous friction, then also the mean momentum of the molecules which have the velocity  $v$  is not equal to  $PBx$ , but to  $\frac{2h}{3} PBxv^2$ . If we leave all of Prof. Tait's calculations completely unaltered, but simply substitute for the expression  $PBx$ , which he introduces in his second paper (p. 260, line 6 from the top), the expression  $\frac{2h}{3} PBxv^2$  here found, we again arrive at O. E. Meyer's formula for the coefficient of friction (*cf.* Part ii. of my 'Theory of Gaseous Friction,' p. 46). We have then applied the right correction at once for the order of magnitude  $B$  to the motion of each molecule, and for the rest may take as the basis of our calculation the special condition of a gas at rest.

(2) A condition of a single gas which is totally different from the special condition must no doubt approach the special condition with the same rapidity as the mean kinetic energy of a molecule becomes equalized in two mixed gases; but it does not follow that any unimportant deviation from the special condition following a certain regularity—as, for example, is produced by gaseous friction—will be equalized with the same rapidity. On this latter point, I believe a conclusion is to be obtained only from the equations which I have given in my older papers, and which allow the calculation of a quantity which is a minimum for the special



condition. The difference between the value which this quantity has for a given condition and this minimum value gives us a measure of how far this condition differs from the special condition; the value of the differential quotient of this quantity by the time gives the rapidity with which this condition approaches the special condition.

To return again to the proof of Avogadro's law, in what was said at the commencement, as well as in all my older papers, no limiting assumption is made with regard to the ratio  $N_1 : N_2$ , but it is always taken for granted that the smaller of these numbers is very large, though the greater may be many times larger. Mr. Burbury\* goes still further, since he maintains that the two propositions given at the commencement of this section also hold good even when of the one kind of gas only a single molecule is present. With this assertion, which of course has simply a mathematical interest but no physical application, I have agreed, and do so still. Since, however, the validity of this assertion is again an entirely new question, on which the correctness of what has been said does not in the least depend, I will not further prolong the controversy by its discussion, and will only remark that the objection of Prof. Tait with reference to the reversal of direction of all the velocities, which cannot take place until the occurrence of the special condition, and then must affect not one only but all the molecules, has already been answered in my paper, "Remarks on some Problems of the Mechanical Theory of Heat"†. From an arbitrarily chosen, not special, condition we pass under Burbury's assumption (possibly not until after a very long time) into the special condition. If, therefore, we were to reverse the directions of all the velocities at the commencement of the selected condition, we should, inversely perhaps, not reach (or only during some time) conditions still further removed from the special condition; we should more probably, also in the reverse direction, reach the special condition.

## § 2. On the Proof of Maxwell's Law of the Distribution of Velocities.

Prof. Tait admits, in his second paper, that his first proof of this law is defective, inasmuch as the reason he gives for it, that  $F(xyz)$  must be the product of three functions, of which one contains only  $x$ , the second only  $y$ , and the third only  $z$ , is not valid. By  $F(xyz) dx dy dz$  we are therefore to under-

\* Burbury, Phil. Mag. ser. 5, vol. xxi. p. 481 (1886).

† Boltzmann, *Wien. Sitzber.* vol. lxxv. sect. ii. (1877).

stand the probability that the component velocities of a molecule estimated along the axes of coordinates lie respectively between  $x$  and  $x+dx$ ,  $y$  and  $y+dy$ , and  $z$  and  $z+dz$ . But in what Prof. Tait has recently said in his second paper on pp. 252 and 253, I see anything but an exact and cogent proof of this. That that group of molecules which he designates as the minority would behave exactly as if it *alone* were shut in between two material planes, and as if all lines of centres were parallel to these planes at the moment of impact, is indeed asserted by Prof. Tait, but without the least proof; for since a molecule of this minority must come into collision with a molecule of the majority infinitely oftener than with another molecule of the minority, in which continually the  $x$ -,  $y$ -, and  $z$ -components of the velocities are exchanged, it is inconceivable why the molecules of the minority should behave as if they alone were present, if we do not assume Maxwell's law of distribution of velocities as already proved. The condition of the molecules of the minority will no doubt be stationary, but also dependent upon the condition of the molecules of the majority. But the relative motion of the molecules of the minority with reference to those of the majority is different for different values of the velocity  $x$ , with which very nearly all the molecules of the minority move parallel to the axis of abscissæ. Hence the relative probability of the different values of the  $y$ - and  $z$ -components of the velocity might also quite well depend upon the value of  $x$  for the minority-molecules in question.

Let us suppose, to take a case chosen at random, that  $x$  is equal to twice the mean velocity of a molecule, or even somewhat larger. Then there will be very few molecules of the majority which have small velocities relatively to those of the minority. Relative to each molecule of the minority, more than half the molecules of the majority will move with a velocity which is equal to the double mean velocity, or still greater. Since this large relative velocity in the impacts is continually transformed into velocity parallel to the Y- and Z-axis, it might quite well be possible that for so large a value of  $x$  amongst the molecules of the minority, large velocity-components in the Y and Z direction would prevail; whereas for small values of  $x$  among the molecules of the minority, the small  $y$ - and  $z$ -components of velocity would predominate. From the behaviour of the different groups of a community, we can therefore only conclude that the number of those molecules of the minority for which the velocity-components estimated along the Y- and Z-axis lie between  $y$  and  $y+dy$  or  $z$  and  $z+dz$ , as the case may be, always remains the same on

the whole, and is therefore independent of the time  $t$ , but not that the ratio of this number to the total number of the minority molecules is independent of  $x$ . I put here, side-by-side with the dictum of De Morgan, this other, that it is just in the calculation of probabilities that one must not be satisfied with general statements, but must sharply prove each assumption at the risk of becoming dull as the result of clear logical developments. The bare expansibility of mathematical developments can hardly be taken to be the test of their applicability to Physics.

But with Prof. Tait's proof of Maxwell's law of distribution of velocities there falls also his proof of Avogadro's law, which implies the first law; or at least it becomes superfluous, since in Maxwell's second and my proof of the first law, Avogadro's law is, without this, proved at once. Another proof of Maxwell's law of distribution of velocities, however, does not yet exist.

(If the gas were supposed to be in constant motion, then we must understand by  $x$  the difference of the component velocities of a molecule from the mean velocity of all the molecules; and the like must hold for  $y$  and  $z$ , in order that the distribution of velocity among the molecules of the minority may be the same function of  $x, y, z$  as with a gas at rest.)

### § 3. *On the Mean Length of Path.*

The mean length of path of a gaseous molecule is most naturally defined, according to Maxwell's proposal, as the arithmetic mean of all the paths which all the molecules in the unit volume describe between one impact and the next. Prof. Tait, on the other hand, proposes (in the first paper of the 'Edinburgh Transactions,' p. 74) to fix attention upon all the molecules in the unit volume at a particular instant, and to observe what path each of these molecules describes from the given moment until it next comes into collision, and of all these paths to take the arithmetic mean. In Maxwell's method we take into the arithmetic mean so many paths of each molecule as it makes impacts in the unit of time; in Tait's method, on the other hand, only a single path of each molecule is counted. Since the swifter molecules come into collision more frequently, and generally describe a longer path from one impact to the next than the slower ones, in the first method the longer paths are counted relatively oftener, and therefore the mean must come out greater than in the second. In order to calculate the mean path according to the first method, we observe all the impacts which each molecule

contained in the unit volume suffers during an entire second, and note all the paths which each molecule describes between two consecutive impacts, and take the arithmetic mean of all these paths. If, according to Prof. Tait's notation, there are  $n$  molecules in the unit volume, of which  $n \cdot n_v$  have a velocity lying between  $v$  and  $v + dv$ , so that  $\sum n_v = 1$ ; and if these latter molecules come into collision with other molecules  $N_v$  times in the second, and describe a mean path  $p_v$ , so that  $N_v \cdot p_v = v$ , then, since  $N_v$  gives at once how many paths each of the  $n \cdot n_v$  molecules describes in the second, the mean path, according to Maxwell's definition, is

$$\lambda_1 = \frac{\sum n n_v N_v \cdot v}{\sum n n_v N_v} = \frac{\sum n_v v}{\sum n_v v : p_v}.$$

This is exactly the formula which Maxwell\*, O. E. Meyer†, &c. use, and which gives  $\lambda_1 = 0.0707\lambda$ . Here  $\lambda$  is the mean path which a molecule would have if all the others, without alteration of magnitude and number, included in the unit volume were at rest. According to the method proposed by Prof. Tait, we have to take of each of the  $n \cdot n_v$  molecules, not  $N_v$  members, but only one member,  $p_v$ , in the arithmetical mean; and so we obtain

$$\lambda_2 = \frac{\sum n n_v p_v}{\sum n n_v} = \sum n_v p_v = 0.677\lambda.$$

In accordance with what has been said, it will be seen that Prof. Tait is decidedly in the wrong if he designates Maxwell's definition of mean path as an erroneous one; on the contrary, it seems to me more natural than either of Prof. Tait's new definitions. For those who care for numerical results, I may remark that twenty years ago, in the course of similar investigations, which remained unpublished, I found

$$\int_0^\infty \frac{4x^3 e^{-x^2} dx}{xe^{-x^2} + (2x^2 + 1) \int_0^x e^{-x^2} dx} = 0.650511,$$

$$\int_0^\infty \frac{4x^4 e^{-x^2} dx}{xe^{-x^2} + (2x^2 + 1) \int_0^x e^{-x^2} dx} = 0.677464,$$

$$\int_0^\infty \frac{4x^5 e^{-x^2} dx}{xe^{-x^2} + (2x^2 + 1) \int_0^x e^{-x^2} dx} = 0.838264,$$

\* Phil. Mag. ser. 4, vol. xix. (1860).

† *Theorie der Gase*, Breslau, 1877, p. 294.



which values are possibly more exact than those given by Prof. Tait. The total number of impacts which all the molecules contained in the unit volume make in a second is  $\sum n_v N_v = \sum n_v v : p_v$ . Only the third of these integrals, as already remarked in § 1, was published in the second part of my "Theory of Gaseous Friction," in formula 9, p. 45. One other circumstance deserves mention. We have spoken all the time as if during the entire second the same  $n \cdot n_v$  molecules would move with a velocity lying between  $v$  and  $v + dv$ . In fact, the velocities of the molecules are constantly changing; but since, on the whole, for a molecule which loses the velocity  $v$ , another gains the same velocity, the above expression does not lead to a false result.

§ 4. *On the Heat-equilibrium of Gases upon which External Forces act.*

Prof. Tait, in his first paper in the Edinburgh Transactions, page 91, also raises objections to my calculation of the heat-equilibrium of a gas on which external forces act, and treats this case differently, starting from an assumption, which he gives in italics on page 92 of the paper referred to at the conclusion of § 31.

We have to distinguish between a *defining-assumption*, the object of which is to define the problem which is to be mathematically handled, and an unproved assertion that, from the defining-assumptions made, certain consequences would follow. If, for example, we assume that the molecules are elastic spheres, that glass walls are rigid elastic planes, &c., these are *defining-assumptions*. In nature these assumptions are certainly not exactly fulfilled, but the logical consequence of the conclusions drawn from them is not thereby affected. But Prof. Tait's above-cited assertion is of the latter kind, since he gives no proof that it is a mathematical consequence of the defining-assumptions made. Also I am sure that my defining-assumptions in many respects correspond extremely imperfectly with the properties of the actual gas. On the other hand, both my proof of Avogadro's law, and my calculation of the heat-equilibrium of a heavy gas, are free from prejudicial and unproved assumptions. In order to show this I will give what seems to me a greatly simplified treatment of this problem, using the results of others which have appeared in the last twelve years upon this subject. And in this I will make use altogether of the method which H. A. Lorentz, in

his treatise "On the Equilibrium of Potential Energy amongst Gaseous Molecules,"\* was the first to employ.

I assume that we have a gas constituted in the usual way entirely of similarly constituted monatomic gaseous molecules, and enclosed in a vessel bounded on all sides by solid walls. For the sake of simplicity we will suppose the form of the vessel unalterable. Let each molecule be a solid absolutely elastic sphere of mass  $m$ , which also is infinitely little deformed upon impact, and whose diameter is vanishingly small in comparison with the mean path. We also suppose the molecules to be reflected against the walls of the vessel as perfectly elastic.

Besides these elastic forces, now let external forces act upon the gaseous molecules, and let  $mX$ ,  $mY$ ,  $mZ$  be the components, estimated along the axes of coordinates of the external force which acts upon a molecule the coordinates of whose centre are  $x$ ,  $y$ ,  $z$ . Let these external forces be independent of the time, but have a potential, and let them be nearly constant within a space of the dimensions of a mean path. We will construct the diagram of velocities by drawing from the centre of coordinates a straight line to represent the velocity of each molecule in direction and magnitude. We will call the end-points of the straight lines thus obtained the "velocity-points" of the corresponding molecules. From all the molecules we choose those for which the coordinates of the centre lie between the limits

$$x \text{ and } x+dx, \quad y \text{ and } y+dy, \quad z \text{ and } z+dz; \quad . \quad (1)$$

and the component-velocities between the limits

$$\xi \text{ and } \xi+d\xi, \quad \eta \text{ and } \eta+d\eta, \quad \zeta \text{ and } \zeta+d\zeta; \quad . \quad . \quad (2)$$

and of these we will say that they lie in the parallelepiped  $dx dy dz$ , and their velocity-point in the parallelepiped  $d\xi d\eta d\zeta$ . The number of these molecules will be denoted by

$$f(x, y, z, \xi, \eta, \zeta, t) d\omega, \quad . \quad . \quad . \quad (3)$$

where

$$d\omega = dx dy dz, \quad d\omega = d\xi d\eta d\zeta.$$

We write here also the time  $t$  under the functional sign in order to include the general case that the gas is in motion under the influence of external forces: for the condition of rest, of course, the function  $f$  must be independent of the time.

Let us now, following Lorentz, imagine a second function

\* H. A. Lorentz, *Wien. Sitzungsber.* vol. xcv. p. 117 (1887).

formed of the seven variables  $x, y, z, \xi, \eta, \zeta, t$ , which we will denote by  $\phi$ . This function may be chosen quite arbitrarily. It may be altogether independent of the function  $f$ , or may be connected with it in any way whatever (*e. g.* it may be identical with  $f$ , or equal to the natural logarithm of  $f$ , &c.). If we substitute in  $\phi$  the coordinates and component velocities of the centre of any molecule, we obtain the value of  $\phi$  corresponding to the molecule in question at the time  $t$ . The sum of the values of  $\phi$  corresponding to all the molecules at the time  $t$  we will denote by  $\Sigma(\phi)$ .

We may then evidently write

$$\Sigma\phi = \int \phi \cdot f \cdot d\omega, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where the sign  $\int$  denotes an integration with reference to the variables  $\xi, \eta, \zeta$  from  $-\infty$  to  $+\infty$ , but with reference to the variables  $x, y, z$  over the whole volume of the vessel. A change of the sum  $\Sigma\phi$  will be produced during an infinitely small time  $\delta t$  by various causes:—

(1) By the function  $\phi$  explicitly containing the time. The change thus produced may be denoted by a prefixed  $\delta_1$ . We have then

$$\delta_1\Sigma\phi = \delta t \int \frac{\partial\phi}{\partial t} \cdot f \cdot d\omega. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

(2) Because the molecule which at the time  $t$  has the coordinates  $x, y, z$  and the velocity-components  $\xi, \eta, \zeta$ , at the time  $t + \delta t$  has the coordinates  $x + \xi\delta t, y + \eta\delta t, z + \zeta\delta t$  and the velocity-components  $\xi + X\delta t, \eta + Y\delta t, \zeta + Z\delta t$ . The whole change in  $\Sigma\phi$  thus produced will be

$$\delta_2\Sigma\phi + \delta_3\Sigma\phi;$$

where

$$\delta_2\Sigma\phi = \delta t \int \left( \xi \frac{\partial\phi}{\partial x} + \eta \frac{\partial\phi}{\partial y} + \zeta \frac{\partial\phi}{\partial z} \right) d\omega, \quad . \quad (6)$$

$$\delta_3\Sigma\phi = \delta t \int \left( X \frac{\partial\phi}{\partial \xi} + Y \frac{\partial\phi}{\partial \eta} + Z \frac{\partial\phi}{\partial \zeta} \right) d\omega. \quad . \quad (7)$$

If we employ the abbreviation

$$\left( \frac{\partial\phi}{\partial t} + \xi \frac{\partial\phi}{\partial x} + \eta \frac{\partial\phi}{\partial y} + \zeta \frac{\partial\phi}{\partial z} + X \frac{\partial\phi}{\partial \xi} + Y \frac{\partial\phi}{\partial \eta} + Z \frac{\partial\phi}{\partial \zeta} \right) \delta t = \delta'\phi, \quad (8)$$

then

$$\delta_1\Sigma\phi + \delta_2\Sigma\phi + \delta_3\Sigma\phi = \int \delta'\phi f \cdot d\omega. \quad . \quad . \quad . \quad (9)$$

Strictly speaking, those molecules ought to be excluded from the integration which during the time  $\delta t$  come into collision with each other. But since the number of these molecules is infinitely small, like  $\delta t$ , and the integral moreover is multiplied by  $\delta t$ , there result thus only terms of the order of magnitude  $(\delta t)^2$ , which may be neglected.

(3) If  $\Sigma\phi$  suffers a change in consequence of the impacts which occur during the time  $\delta t$ , this change also, which we will denote by  $\delta_i\Sigma\phi$ , must be taken into account. I proceed exactly as Lorentz does in the paper referred to. We choose from all the impacts which occur in the parallelepiped  $dx dy dz$  during the time  $\delta t$  only those for which, before impact, the velocity-components of one of the impinging molecules lie between the limits (2), whilst the velocity of the centre of gravity of the impinging molecules lies between the limits

$$u \text{ and } u+du, \quad v \text{ and } v+dv, \quad w \text{ and } w+dw; \quad . \quad (10)$$

and, lastly, the direction of the line of centres of both molecules for the impacts chosen at the moment of impact lies within an infinitely small cone of given direction in space and of aperture  $d\lambda$ . If  $\sigma$  denotes the diameter of the molecules,  $V$  their relative velocity, and  $\theta$  the acute angle of the directions  $V$  and  $C$ , then the number of selected impacts is

$$dn = \sigma^2 f f_1 V \cos \theta \, d\sigma \, d\omega \, d\lambda \, \delta t; \quad . \quad . \quad . \quad (11)$$

where

$$f = f(x, y, z, \xi, \eta, \zeta, t), \quad f_1 = f(x, y, z, u-\xi, v-\eta, w-\zeta, t), \\ dp = du \, dv \, dw \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Since we have assumed the masses equal, the velocity-components of the second of the impinging molecules before impact differ infinitely little from  $u-\xi$ ,  $v-\eta$ ,  $w-\zeta$ . After impact the velocity-components of the first of the impinging molecules must lie between the limits

$$\xi' \text{ and } \xi' + d\xi', \quad \eta' \text{ and } \eta' + d\eta', \quad \zeta' \text{ and } \zeta' + d\zeta'; \quad . \quad (13)$$

those of the second molecule then differ infinitely little from  $u-\xi'$ ,  $v-\eta'$ ,  $w-\zeta'$ .

The function  $\phi$  for the two molecules before impact has then the values

$$\phi(x, y, z, \xi, \eta, \zeta, t) \text{ and } \phi(x, y, z, u-\xi, v-\eta, w-\zeta, t),$$

for which we will write  $\phi$  and  $\phi_1$  for the sake of brevity. But



after impact  $\phi$  has for the same molecules the values

$$\phi(x, y, z, \xi', \eta', \zeta', t) \text{ and } \phi(x, y, z, u - \xi', v - \eta', w - \zeta', t),$$

for which we will write  $\phi'$  and  $\phi_1'$ . Infinitely small magnitudes of the order  $\delta t$  may be neglected (so that in the two last expressions, for example,  $t$  may be written instead of  $t + \delta t$ ), since the quantities  $\phi$ ,  $\phi_1$ ,  $\phi'$ , and  $\phi_1'$  occur in equation (18) multiplied by a factor which is itself an infinitely small quantity of the order  $\delta t$ . By each of these impacts, therefore,  $\Sigma\phi$  is diminished by  $\phi + \phi_1$ ; on the other hand, it is increased by  $\phi' + \phi_1'$ . By all the selected impacts together, therefore,  $\Sigma\phi$  is increased by

$$dn(\phi' + \phi_1' - \phi - \phi_1).$$

We will now consider, together with the action of the hitherto selected impacts, the action of those which in the volume-element  $do$  during the time  $\delta t$  have acted in exactly the opposite way, and which, for shortness, we will call the reversed impacts. These latter, therefore, will be just those for which the velocity-components of the centre of gravity and the position of the line of centres of the spheres at the moment of impact lie between the same limits as for the first selected, but for which the velocity-components of the first impinging molecule at the moment at which the impact begins lie between the limits (13); so that those of the second of the impinging molecules differ infinitely little from  $u - \xi'$ ,  $v - \eta'$ ,  $w - \zeta'$ . On the other hand, as we at once see, for the reversed impacts the velocity-components of the first of the impinging molecules after impact lie between the limits (2); but those of the second differ infinitely little from  $u - \xi$ ,  $v - \eta$ ,  $w - \zeta$ . By each of the reversed impacts, therefore,  $\Sigma\phi$  is increased by

$$\phi + \phi_1 - \phi' - \phi_1';$$

and if we denote the total number of reversed impacts by  $dn'$ , then during the time  $\delta t$  their effect is to increase  $\Sigma\phi$  by the amount

$$dn'. (\phi + \phi_1 - \phi' - \phi_1').$$

This quantity, therefore, by all the first selected impacts and the corresponding reversed impacts, is increased by

$$\Delta = (dn - dn')(\phi' + \phi_1' - \phi - \phi_1). \quad . \quad . \quad . \quad (14)$$

Exactly as in formula (11) we have next

$$dn' = \sigma^2 f' f_1' V \cos \theta \, do \, d\omega' \, dp \, d\lambda \, \delta t, \quad . \quad . \quad (15)$$

where

$$f' = f(x, y, z, \xi', \eta', \zeta', t), \quad f_1' = f(x, y, z, u - \xi', v - \eta', w - \zeta', t), \\ d\omega' = d\xi' d\eta' d\zeta'.$$

In order to express  $d\omega'$  in terms of  $d\omega$ , we must imagine the magnitude and position of the centre of gravity as well as the direction of the line of centres at the moment of impact completely fixed. On the other hand, the velocity-components of the first of the impinging molecules may vary between the limits (2). In the accompanying figure, in which the rectangle  $\xi\xi'CC'$  does not necessarily lie in the same plane as the triangle  $O\xi u$ , let  $O\xi$  be the velocity of the first molecule before impact,  $Ou$  the velocity of the centre of gravity of both molecules, and  $\xi C'$  the direction of the line of centres at the moment of impact. The straight lines (not drawn in the figure)  $O\xi'$ ,  $OC$ , and  $OC'$  therefore represent respectively the velocity of the first molecule after impact, that of the second before, and that of the second after impact. We have now to keep the points  $O$  and  $u$ , and the direction of  $\xi C'$  unchanged, whereas the point  $\xi$  must describe the whole interior of the parallelepiped  $d\omega$ . Since here the figure  $\xi\xi'CC'$  is always a rectangle, and  $u$  always remains its centre, we see at once that the point  $\xi'$  describes a parallelepiped  $d\omega'$  congruent with the parallelepiped  $d\omega$ , which represents nothing else than the product of the differentials  $d\xi' d\eta' d\zeta'$ ; consequently  $d\omega = d\omega'$ . (I have given a much more complete account of this transformation of coordinates and its relationship to various formulæ of the kinetic theory of gases in my paper "On the Assumptions necessary for the Proof of Avogadro's Law."\* H. A. Lorentz attains the greatest simplicity by using the velocity-components of the centre of gravity instead of the components of the relative velocity which I have employed on page 641.) We therefore obtain from formula (15),

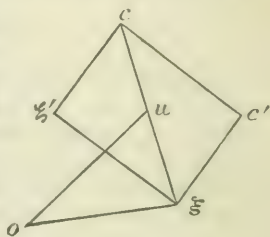
$$dn' = \sigma^2 f' f_1' V \cos \theta \, d\omega \, dp \, d\lambda \, \delta t; \quad \dots \quad (16)$$

and from formula (14),

$$\Delta = (\phi' + \phi_1' - \phi - \phi_1)(f' f_1' - f f_1) \sigma^2 V \cos \theta \, d\omega \, dp \, d\lambda \, \delta t. \quad (17)$$

In order to find  $\delta_4 \Sigma \phi$  from this, we have to integrate this expression with reference to all differentials denoted by  $d$  over

\* Boltzmann, *Wien. Sitzungsber.* vol. xciv. p. 637 (1886); *Phil. Mag.* [5] vol. xxiii. p. 305.



all possible values, which we will express by a single integral sign.

In this, however, we must observe that, after performing the integration, we have counted each impact four times. Because we have extended the integration over all the first selected impacts, we have already counted all the impacts once; by further extending the integration over all the reversed impacts, we have counted all the impacts a second time; moreover, both in the first and in the second integration each separate impact is counted twice, since the molecule with the velocity-components  $\xi \eta \zeta$  may be both the first as well as the second of the impinging molecules.

Therefore, on the whole,

$$\delta_4 \Sigma \phi = \frac{1}{4} \sigma^2 \delta t \int (\phi' + \phi_1' - \phi - \phi_1) (f f_1 - f' f_1') V \cos \theta \, d\theta \, d\omega \, dp \, d\lambda. \quad (18)$$

The whole increase  $\delta \Sigma \phi$ , which  $\Sigma \phi$  undergoes during the time  $\delta t$ , is the sum of the expressions (5), (6), (7), and (18). Since the function  $\phi$  was left undetermined, there is nothing now to prevent our putting

$$\phi = lf,$$

where  $l$  denotes the natural logarithm. Then we shall have

$$\delta_1 \Sigma \phi = \delta t \int \frac{\partial f}{\partial t} \cdot d\theta \, d\omega. \quad . \quad . \quad . \quad . \quad (19)$$

This is simply the change which the total number of molecules contained in the space suffers in the time  $\delta t$ ; and is evidently equal to zero, since we assume that the walls of the vessel are solid.

According to formula (6),  $\delta_2 \Sigma \phi$  consists of three summations. Integrating the first of these with reference to  $x$ , the second with reference to  $y$ , and the third with reference to  $z$ , we obtain

$$\delta_3 \Sigma \phi = \delta t \int f n \, ds \, d\omega; \quad . \quad . \quad . \quad . \quad (20)$$

where  $ds$  is a surface-element of the vessel,  $n$  the velocity-component of a molecule normal to the surface-element in question.

We may now for each surface-element  $ds$  introduce three new variables instead of  $\xi, \eta, \zeta$ , namely the components of the velocity in the direction of normals to the vessel, and in two directions at right angles, which we will denote by  $n, q, r$ . Integral (20) then becomes

$$\delta t \int f n \, ds \, dn \, dq \, dr, \quad . \quad . \quad . \quad . \quad (20a)$$

which is to be integrated with reference to  $n, q, r$  from  $-\infty$  to  $+\infty$ . Since we assume that the molecules are reflected at the solid walls of the vessel like elastic spheres, those molecules which strike against the walls of the vessel with certain values of  $q, r$  will return from the wall with the same values of  $q$  and  $r$ . The impinging molecules differ from the returning molecules only in the sign of  $n$ . The function  $f$  therefore remains unaltered if  $q$  and  $r$  remain unaltered and only  $n$  changes its sign, whence it follows that in the integral (20a) each two terms neutralize each other; consequently this integral itself has the value 0. So also of the three summations, of which, according to equation (7), the quantity  $\delta_3 \Sigma \phi$  consists, we may integrate the first with reference to  $\xi$ , the second with reference to  $\eta$ , and the third with reference to  $\zeta$ , between the limits  $-\infty$  and  $+\infty$ . But since for infinite values of the velocities  $f$  must necessarily vanish, we see that also  $\delta_3 \Sigma \phi = 0$ .

If, at the beginning of the time, the gas were contained in a finite space not enclosed by walls (the integration-space), then the expression (19) would be negative, and numerically equal to the number of the molecules which emerge from the integration-space during the time  $\delta t$ . But then the number of these molecules would be exactly equal to the expression (20), so that  $\delta_1 \Sigma \phi + \delta_2 \Sigma \phi$  would still be equal to zero. So also  $\delta_1 \Sigma \phi$  would assume a negative value, and  $\delta_3 \Sigma \phi$  an equal positive value, if the velocities of the molecules at the beginning of the time were included within finite limits.

If we wish to prove the equation

$$\delta_1 \Sigma \phi + \delta_2 \Sigma \phi + \delta_3 \Sigma \phi = 0 \quad . \quad . \quad . \quad . \quad . \quad (21)$$

directly, without employing integration with reference to one of the coordinates or velocities, we may proceed thus:—Let us imagine each point of the parallelepiped  $do$  moved forward in space with the velocity-components  $\xi, \eta, \zeta$ , and each point of the parallelepiped  $d\omega$  with the velocity-components  $X, Y, Z$ ; whereby the first points of course are reflected at the walls exactly like the molecules, and the latter also must be correspondingly altered.

Since  $X, Y, Z$  are not functions of  $\xi, \eta, \zeta$ , neither of the parallelepipeds alters its size. Let the parallelepipeds be denoted in their original position by  $do$  and  $d\omega$ , and in their new position, which they have reached by the motion of their points during the time  $\delta t$  by  $do^*$  and  $d\omega^*$ . Let  $\phi$  be the value of this function at the time  $t$ , when in it the values are substituted for the variables which correspond to the middle points of the parallelepipeds  $do$  and  $d\omega$ , whilst this function



at the time  $t + \delta t$  assumes the value  $\phi^*$ , if for the variables the values are substituted which correspond to the middle points of  $do^*$  and  $d\omega^*$ ; then

$$\phi^* - \phi = \delta t \left( \frac{\partial \phi}{\partial t} + \xi \frac{\partial \phi}{\partial x} + \eta \frac{\partial \phi}{\partial y} + \zeta \frac{\partial \phi}{\partial z} + X \frac{\partial \phi}{\partial \xi} + Y \frac{\partial \phi}{\partial \eta} + Z \frac{\partial \phi}{\partial \zeta} \right),$$

which quantity we have already denoted by  $\delta' \phi$  in equation (8).

According to equation (9),

$$\delta_1 \Sigma \phi + \delta_2 \Sigma \phi + \delta_3 \Sigma \phi = \int \delta' \phi f do d\omega.$$

If, now, we put  $\phi = lf$ , we have

$$\delta_1 \Sigma \phi + \delta_2 \Sigma \phi + \delta_3 \Sigma \phi = \int \delta' f do d\omega = \int f^* do^* d\omega^* - \int f do d\omega.$$

Since in the last expression both the first and second integrals express the total number of the molecules of the gas, their difference must be zero.

The indices and prefixed symbol  $\delta'$  have for the function  $f$  exactly the same meaning as before for the function  $\phi$ .

Hence, if we put  $\phi = lf$ ,  $\delta \Sigma \phi$  reduces to  $\delta_4 \Sigma \phi$ ; and we obtain, if we make use of equations (4) and (18), the equation

$$\delta \int f lf do d\omega = \frac{1}{4} \sigma^2 \delta t \int V \cos \theta do d\omega dp d\lambda \cdot (ff_1 - f'f'_1) \cdot i \left( \frac{f'f'_1}{ff_1} \right). \quad (22)$$

We will also make use of the general equation for the change of the function  $f$ , which I have called equation (44) in my "Further studies on the Heat-equilibrium among Gas-molecules,"† and equation (2) in my paper "On the Heat-equilibrium of Gases upon which External Forces act"‡. Although in the last-named paper I have proved this equation at length, yet I will shortly deduce it from the data before us. The number of molecules whose centres at the time  $t$  lie in the parallelepiped  $do$ , and whose velocity-points at the same time lie in the parallelepiped  $d\omega$ , is

$$N_1 = f(x, y, z, \xi, \eta, \zeta, t) do d\omega, \quad . \quad . \quad . \quad (23)$$

for which, again, we will write for shortness  $f do d\omega$ ; so also the number of molecules for which, at the time  $t + \delta t$ , the centre lies in the parallelepiped  $do^*$ , and the velocity-point in the parallelepiped  $d\omega^*$ :—

$$\begin{aligned} N_2 &= f(x + \xi \delta t, y + \eta \delta t, z + \zeta \delta t, \xi + X \delta t, \eta + Y \delta t, \zeta + Z \delta t, t + \delta t) do^* d\omega^* \\ &= do^* d\omega^* \left[ f + \delta t \left( \frac{\partial f}{\partial t} + \xi \frac{\partial f}{\partial x} + \eta \frac{\partial f}{\partial y} + \zeta \frac{\partial f}{\partial z} + X \frac{\partial f}{\partial \xi} + Y \frac{\partial f}{\partial \eta} + Z \frac{\partial f}{\partial \zeta} \right) \right]. \quad (24) \end{aligned}$$

† Boltzmann, *Wien. Sitzb.* vol. lxvi. (1872).

‡ Ibid. vol. lxxii. (1875).

The number of molecules of the  $N_1$  molecules which, during the time  $\delta t$ , come into collision with any others, is obtained by integrating the expression given by equation (11) for  $dn$  with reference to all possible values of  $du$ ,  $dv$ ,  $dw$ , and  $d\lambda$ . The result of this integration may be written thus :—

$$N_3 = do \, d\omega \, \sigma^2 t \int f f_1 V \cos \theta \, dp \, d\lambda. \quad . \quad . \quad (25)$$

Besides these  $N_3$  molecules, all the molecules whose number is denoted above by  $N_1$ , after lapse of the time  $\delta t$ , reach the parallelepiped  $do^*$ , and their velocity-points the parallelepiped  $d\omega^*$ .

Of the molecules whose centres are in the parallelepiped  $do$  there are, however, during the time  $\delta t$  some (let their number be  $N$ ) which had before altogether other velocities, but by impact with other molecules have attained exactly such velocities that their velocity-point after impact lies in the parallelepiped  $d\omega^*$ . We have therefore  $N_2 = N_1 - N_3 + N_4$ . Observing equations (23), (24), and (25), and remembering that  $N_4$  was obtained from equation (16) by integration exactly as  $N_3$  from equation (11), we obtain, since, as already remarked,  $do^* = d\varrho$  and  $d\omega^* = d\omega$ ,

$$\begin{aligned} \frac{\partial f}{\partial t} + \xi \frac{\partial f}{\partial x} + \eta \frac{\partial f}{\partial y} + \zeta \frac{\partial f}{\partial z} + X \frac{\partial f}{\partial \xi} + Y \frac{\partial f}{\partial \eta} + Z \frac{\partial f}{\partial \zeta} \\ = \sigma^2 \int (f' f_1' - f f_1) V \cos \theta \, dp \, d\lambda. \quad . \quad (26) \end{aligned}$$

The right-hand side of equation (22) is a sum of terms of which no one can be positive. But since, as soon as the gas is at rest and does not change its condition, the left-hand side must vanish, so also each term of the right-hand side must vanish by itself ; and we have generally for any values whatever of the variables  $f f_1 = f' f_1'$ .

Since this equation must hold for all possible values of the variables occurring in it, so long as only the condition

$$\xi^2 + \eta^2 + \zeta^2 = \xi'^2 + \eta'^2 + \zeta'^2 \quad . \quad . \quad . \quad (27)$$

is fulfilled, it follows at once in the usual way that  $f$ , if the gas be at rest, must have the form

$$F e^{-h(\xi^2 + \eta^2 + \zeta^2)}, \quad . \quad . \quad . \quad (28)$$

where  $h$  is a constant, and only  $F$  involves the variables  $x, y, z$ . In order to determine  $F$  we make use of equation (26). We see at once that since  $f$  has the form (28), the expression  $f' f_1' - f f_1$  must always vanish, since equation (27) must be satisfied for every impact. Therefore also the right-hand side of equation (26) must vanish, and the substitution of the

value (28) gives

$$\xi \frac{\partial F}{\partial x} + \eta \frac{\partial F}{\partial y} + \zeta \frac{\partial F}{\partial z} + 2hF(\xi X + \eta Y + \zeta Z) = 0.$$

Since now the magnitude  $F$  can no more contain the variables  $\xi, \eta, \zeta$ , the term multiplied by  $\xi$ , that multiplied by  $\eta$ , and that multiplied by  $\zeta$  in the last equation must separately vanish, whence at once the well-known hydrostatic differential equations follow. Consequently this line of argument, which Prof. Tait designates a "remarkable proceeding," does not in any way involve any special assumption, but is a logical treatment of the general equations of the kinetic theory of gases; and I am confident that those of my readers who honour the foregoing with sufficient attention will not be able to raise any objection to it. (*Cf.* on this point also Maxwell, 'Nature,' vii. pp. 525 & 535, October 23, 1873.)

## XII. *The Recalescence of Iron.*

By HERBERT TOMLINSON, B.A.\*

### *Introduction.*

MANY specimens of iron and steel, when they have been raised to a white heat and are cooling, exhibit at a certain stage a remarkable phenomenon—the metal, to all appearances, receives a sudden accession of heat, and reglows. This phenomenon was discovered by Professor Barrett, and is frequently designated the "recalescence" of iron. The author believes the recalescence of iron to be due to a sudden *physical* change, the event of which has been retarded by what he will call "subpermanent retentivity." The retentivity of a substance may be defined to be that property by virtue of which the substance does not *immediately* recover from the strain produced by a stress when the stress has been removed. If the original strain produced by the stress be large, the residual strain will consist of two parts: one permanent, which will not disappear even after the lapse of any length of time; the other subpermanent, which will disappear after a greater or less interval of time, and which may be aided in doing so by molecular vibrations set up by mechanical or any physical agency. Subpermanent retentivity appears with a great variety of strains:—when a wire is twisted or bent, when a piece of iron or steel is magnetized, when a Leyden jar is charged, and when glass has been raised in temperature. The last is an example of subpermanent

\* Communicated by the Physical Society: read December 10, 1887.

strain resulting from thermal agency, and is the strain with which we have now to deal. Such a strain is not peculiar to glass; for the author has found that a coil of zinc wire, which had been previously heated to nearly the melting-point of the metal and afterwards suddenly cooled by plunging into cold water, continued to show decrease of volume for some minutes after the cooling. The internal friction of a metal seems to be intimately connected with the subpermanent retentivity; and the author has already investigated the internal friction of several metals at various temperatures\*. The results of the above-mentioned investigation show that changes of temperature, even when they do not exceed  $100^{\circ}$  C., exercise a very considerable effect on the internal friction; and it seemed likely that a preliminary examination of the internal friction of iron at very high temperatures might be of service in elucidating the problem of recalescence.

*The Internal Friction of Iron at High Temperatures.*

*Experiment I.*—In fig. 1, *ab* is an iron wire 49·5 centim. in length and ·041 centim. in diameter; the ends *a* and *b* are brazed to two pieces of rather stout brass wire; the upper piece of brass wire is furnished with a binding-screw, *S*, and the lower one is soldered to the centre of a hollow horizontal brass bar, *V*; *D* is a piece of copper wire soldered at its upper extremity to *V*, and dipping with its lower extremity into mercury contained in a glass vessel, *H*. The wire hangs in the axis of a glass tube, *T*, fitting into the cover of a box, *B*; the tube is closed with a cork, *C*, and the box *B*, though provided with a door which can be opened for making adjustments or for starting the torsional oscillations, is kept shut up during the actual observations. The box is also provided with a glass window, which enables an illuminated circle crossed by a fine vertical dark line to be thrown on to the light mirror, *M*, and hence be reflected back on to a scale according to the usual arrangement. The wire was set in torsional oscillation by means of a light feather gently pressed against one end of *V* and then removed†; the amplitudes of the vibrations were never allowed to exceed one degree.

The wire was heated by means of an electric current from a battery of 30 Grove cells, an ampere-meter and a set of resistances serving respectively to measure and to vary the

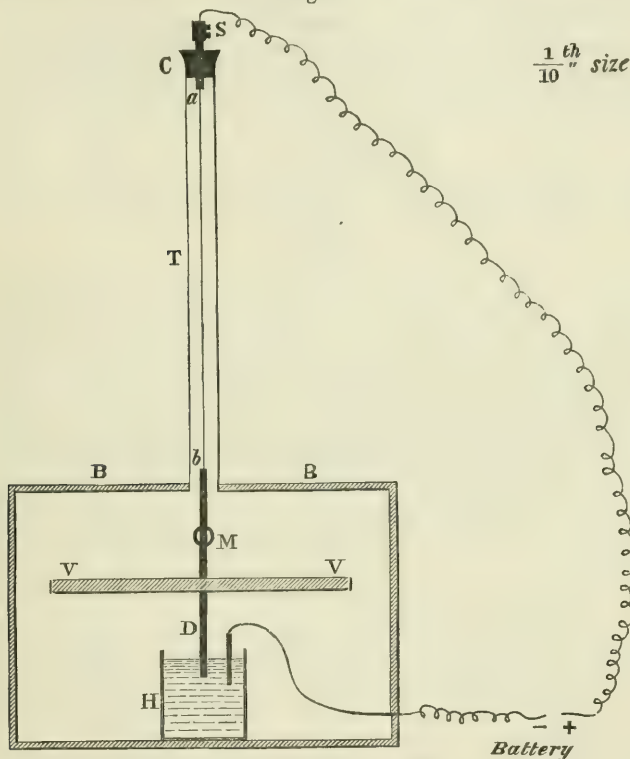
\* Phil. Trans. vol. clxxvii. (part ii. 1886) p. 801. Proc. Roy. Soc. No. 244 (1886), p. 343.

† It would have been better to have had a small piece of iron attached to *V* and to have started the vibrations by a magnet on the outside of the box; since the amplitudes diminished very rapidly at the higher temperature.



strength of the current. The internal friction of the iron was measured by the logarithmic decrement of arc of the wire when set in torsional oscillation. Part of the whole observed decrement is due to the friction of the air, and part to the

Fig 1.



friction of the wire D against the mercury in H. The former of these can be calculated from formulæ given in the author's paper on the internal friction of metals\*; the latter by observing the logarithmic decrement at the temperature of the room, first when D dips into the mercury, and again when it does not, whilst from this again the effect of the friction of the mercury at higher temperatures can be approximately determined provided the change wrought in the torsional elasticity be known. The logarithmic decrement resulting from the friction of the air and the friction of the mercury conjointly was small compared with the whole observed decrement, and was in all the calculations allowed for.

\* Phil. Trans. vol. clxxvii. (part ii. 1886) pp. 813-815.

The logarithmic decrement due to internal friction was determined in the first instance before the wire had been heated at all, and proved to be

$$\cdot 002203^*.$$

The wire was now heated to the highest temperature recorded in this experiment, and was maintained for about half an hour at this temperature, being all the time kept in torsional oscillation; it was then allowed to cool, and immediately after cooling was again tested. The logarithmic decrement now proved to be

$$\cdot 019367.$$

After a rest of one hour the logarithmic decrement had considerably diminished, and finally fell to

$$\cdot 00977.$$

Lastly, the wire was again raised to the previous high temperature, and as soon as the logarithmic decrement and vibration-period had become sensibly constant† their values were registered. The temperature was then lowered by putting resistance into the battery-circuit, and a fresh set of observations was taken, and so on, until eventually the temperature of the wire was reduced to that of the room, when the logarithmic decrement proved to be sensibly the same as the one last recorded. The results of the observations, which extended over a period of several days, are given in the following table :—

Time of vibration, in seconds.	Modulus of torsional elasticity, in grammes per square centim.	Temperature, in degrees Centigrade, $t$ .	Logarithmic decrement due to internal friction, $\lambda$ .	Difference between consecutive temperatures, $\Delta t$ .	Difference between consecutive values of $\lambda$ , $\Delta \lambda$ .	$\Delta \lambda : \Delta t$ .
1.523	$726.6 \times 10^6$	20	$\cdot 00977$			
1.573	681.1	220	$\cdot 01805$	200	$\cdot 00828$	$\cdot 000041$
1.700	583.2	543	$\cdot 05033$	323	$\cdot 03228$	$\cdot 000100$
2.020	413.1	930	$\cdot 09634$	387	$\cdot 04601$	$\cdot 000119$
2.100	382.2	989	$\cdot 11216$	59	$\cdot 01582$	$\cdot 000268$
2.486	272.7	1181	$\cdot 20950$	192	$\cdot 09734$	$\cdot 000507$
2.600	249.4	1220	$\cdot 19680$	39		

\* The wire was unannealed, otherwise the logarithmic decrement would have been probably less than half of the amount here given.

† Any change of temperature, whether in the direction of increase or decrease, is always attended in the first instance with a larger internal friction than that which ultimately prevails when the vibrations are continued long enough.

The temperatures given in the third column of the table are deduced from the values of the modulus of torsional elasticity, given in the second column, by means of the formula

$$z_t = z_0(1 - \cdot 0002442t - \cdot 0000002510t^2)^*,$$

where  $z_t$  and  $z_0$  are the values of the modulus at  $t^\circ \text{C.}$  and  $0^\circ \text{C.}$  respectively. This formula was itself deduced from some very careful observations of the torsional elasticity of iron at different temperatures between  $0^\circ \text{C.}$  and  $100^\circ \text{C.}$ ; but the numbers given in the third column must only be regarded as rough approximations to the true values; inasmuch as in the first place the above formula was calculated from observations made with a different specimen of annealed iron, and in the second it by no means follows that the formula would apply for such high temperatures as are here recorded. It would seem, however, from the last column of the table, that somewhere near  $550^\circ \text{C.}$  there begins to be a rapid rise in the internal friction of the iron, and another still more rapid rise at a temperature of about  $1000^\circ \text{C.}$  At this last temperature the internal friction becomes so great that only two or three vibrations are made by the wire before it comes practically to rest; and it is necessary to multiply the observations very considerably before any approach to accuracy in the results can be attained. Some idea of the enormous friction encountered by the molecules may be arrived at by bearing in mind that the logarithmic decrement of a torsionally vibrating iron wire at  $1000^\circ \text{C.}$  is *ten times* the logarithmic decrement of a tin wire at a temperature of  $20^\circ \text{C.}$ , tin possessing the largest internal friction of any metal yet examined at ordinary temperatures.

From the last line in the table it would seem that the internal friction of iron begins to decrease as the temperature rises from  $1100^\circ \text{C.}$  to  $1200^\circ \text{C.}$ ; but the numbers here given must be received with some caution in so far as they are to be used for determining this point†.

We tread on surer ground when we compare the second and fourth columns together; and it is abundantly evident that the decrease of torsional elasticity which ensues on rise of temperature bears no comparison with the increase of internal friction. It is a common lecture-experiment to heat a bell by means of a burner placed underneath, and

\* Proc. Roy. Soc. No. 244 (1886), p. 343.

† A second set of experiments made with another piece of the same wire showed, however, a similar decrease of internal friction but at rather a lower temperature.

to demonstrate that all musical sound departs from the bell when struck provided the temperature approach a visible red. This absence of musical sound is generally, if not always, attributed to diminution of elasticity; but it would seem, from the above, to be much rather attributable to increase of internal friction\*.

Another point to be noted in this experiment is the large permanent increase of internal friction produced by the rise of temperature. This doubtless arose from the fact that, though the mass of the suspended system only amounted to 40 grms., there was a sensible permanent lengthening of the wire resulting from the rise of temperature; for the author has shown that permanent extension and permanent torsion may both increase very considerably the value of the logarithmic decrement.

An equally important point to the above is the effect of rest on the logarithmic decrement when the wire has recently had its temperature changed. It seems not improbable that, large as the internal friction is at the higher temperatures, it would be larger if tested *immediately* after the change had taken place. Again, it would be larger still if tested by a statical method; for the preliminary vibrations, to which the wire was subjected before testing, evidently reduced considerably the internal friction.

### *The Two Critical Temperatures of Iron.*

The above experiment seems to show, not only that the internal friction of iron is very considerable at high temperatures, but also that there are two points, namely about  $550^{\circ}\text{C}$ . and  $1000^{\circ}\text{C}$ ., at which there are very sudden changes in the rate at which the friction increases with the temperature. Now at or near these points there are, at any rate for annealed iron wire of good quality, very notable and sudden changes in certain of the physical properties of the metal. About  $550^{\circ}\text{C}$ . the metal begins to lose its magnetic properties very rapidly; so rapidly indeed that, if an iron wire be surrounded by a magnetizing solenoid always kept in action, and this again be concentric with a secondary solenoid connected with a galvanometer, a very sensible induced current can be observed at the critical temperature both on heating and cooling. It would seem likely also, from the researches of Prof. P. G. Tait and others, that at this temperature, or *about* this temperature, there are sudden changes in the thermoelectrical properties of

\* The elasticity of iron at  $1000^{\circ}\text{C}$ . is nearly the same as the elasticity of silver at  $20^{\circ}\text{C}$ .



the metal and also in the electrical resistance. These sudden changes would appear to indicate an equally sudden change in the molecular architecture of the iron ; and it seems reasonable to suppose that when, on heating, the critical temperature is reached, a certain amount of the whole thermal energy which may be imparted by an electric current or a burner to the wire is used up in producing this change, whilst an equal amount of thermal energy is given out when the wire, on cooling, reassumes its original molecular structure. It by no means follows, however, that the so-called latent heat connected with the sudden change can be detected unless by very refined apparatus ; for it is not at all unlikely that the sudden loss of magnetic properties may be due simply to a rotation of the molecules about their axes ; and the researches of Professor Ewing\* prove that there may be considerable rotation of the molecules about their axes, and yet at the same time very minute change of temperature resulting from this rotation.

Near the higher temperature of  $1000^{\circ}$  C. there is again a most remarkable alteration in the behaviour of an annealed iron wire which is being heated when under stress or strain. If it is stretched by a slight weight it suddenly unstretches † ; if it is under a slight bending-stress it suddenly unbends ; and if it is under slight twisting-stress it suddenly untwists ; whilst, on the contrary, if it has been previously bent or twisted permanently and then entirely released from stress, it suddenly bends more or twists more as the case may be ‡. Further, there are sudden changes, evidently at the same temperature, in the thermoelectrical relations which exist between strained and unstrained iron, and also between stressed and unstressed iron ; these changes, like those before mentioned, being opposite in direction for stress and strain §. All these phenomena indicate, according to the author's belief, a second notable change in the molecular architecture of the iron ; they certainly seem to indicate a change which must involve the expenditure of thermal energy to produce it when the iron is being heated, and the giving out of thermal energy when the iron is being cooled : in other words, here must be, one would think, a second point at which heat becomes latent.

Now suppose that the iron has been raised in temperature above  $1000^{\circ}$  C. and is cooling : when the critical temperature is reached the molecules would begin to come back to their

\* Phil. Trans. part ii. p. 553 (1885).

† Phil. Mag. [4] vol. xlv. p. 472.

‡ Ibid. [5] vol. xxiv. No. 148, p. 256 (1887).

§ Ibid. [5] vol. xxv. No. 152, p. 45 (1888).

original positions, just as water begins to freeze when the temperature of  $0^{\circ}\text{C.}$  has been reached; but internal friction prevents this, and it is not till the temperature has fallen, it may be very considerably, below the critical temperature that the readjustment takes place. When, however, the change under these conditions does take place it will be what Clerk Maxwell has designated as an explosive change, and the energy set at liberty by the transformation will accelerate the subsequent rate of transformation. Here, again, we have an analogy in the case of freezing water; for in a Wollaston's cryophorus the temperature of the water may frequently be reduced several degrees below  $0^{\circ}\text{C.}$  before solidification sets in; but when it does, the whole mass, or a considerable portion of the whole mass, is frozen instantly. With both iron and water, therefore, there will be a greater amount of heat *suddenly* generated when the transformation takes place below the critical temperatures than when it occurs at the critical temperatures. Again, another analogy may be pointed out between ice at  $0^{\circ}\text{C.}$  and iron at  $1000^{\circ}\text{C.}$ ; in both there is a marked change in the cohesion of the molecules. When an iron wire is under any but an exceedingly small stress of bending, torsion, or traction, it begins to yield permanently in a most astonishing manner when, on heating, the temperature reaches  $1000^{\circ}\text{C.}$ ; the metal suddenly becomes, as it were, in a partially fluid condition.

According to the author's view, then, recalescence in cooling iron is owing to retardation, produced by internal friction, of a *physical* change somewhat resembling the change which occurs when water becomes ice: were it not for internal friction the change would take place comparatively in a gradual manner, and a certain amount of thermal energy would be equally gradually given out. In consequence, however, of internal friction the change is retarded until a temperature is reached below the temperature at which it would otherwise occur; when, partly owing to diminution of internal friction and partly to increased internal molecular stress, the molecules give way at some one point, and this is followed by a giving way throughout the whole mass; the change thus partakes of the nature of an explosion, and, as a consequence, there is a rapid rise of temperature. The following experiments were made with the object of collecting evidence for or against the above-mentioned theory.

#### *Experiments on Recalescence.*

It has been observed that there are *two* critical temperatures—one at about  $550^{\circ}\text{C.}$ , and the other at about  $1000^{\circ}\text{C.}$ ;

so that, according to the above theory, there should be at least *two* points at which a sudden *reheating* takes place ; but not necessarily two sensible *reglows*\*, or even, as we have seen, two *sensible* reheatings. This is all the more to be expected ; because when, on heating, a wire under moderate stress has attained a temperature of about  $550^{\circ}$  C., there is evidently a sudden *permanent* yielding showing sudden softening of the metal, though this is not nearly so marked as the permanent yielding which takes place at the higher critical temperature. The next experiment *appeared* to show, not one recalescence, but several.

*Experiment II.*—The end of a steel poker was heated to a very bright red in a fire and was then taken to a corner of the room on one side of the fire. The room was quite dark save where it was faintly lighted up by the fire, and the eyes of the observer had been previously rested by shutting them for five minutes before the experiment. The heated part of the poker was intently watched whilst it was cooling and nothing was observed of any note for some little time. Presently, however, the external surface *appeared* to lose in temperature rapidly, and shortly afterwards to brighten most perceptibly. The apparent rapid darkening and reglow occurred no less than *seven* times during this same cooling. The experiment was repeated again and again, not only on the same night but on several nights, and as often as it was repeated *seemed* to show most conclusively that there were several reglows. It was noticed, however, that when the poker was loosely held in the hand the apparent reglows occurred more frequently than when it was fixed ; and further that, when the outside of the poker was seen to darken, a slight motion of the poker in any direction caused it to brighten. Evidently convection-currents of air could not account for the phenomenon ; for though a current of air might cause a sensible darkening of the external surface by its cooling effect, it could not cause the brightening which seemed to result from the motion. As it was thought that the brightening might be produced by the motion of the iron through the earth's magnetic field, the poker was reheated and fixed in a horizontal position above the poles of a rather powerful electromagnet. As soon as one of the darkenings preliminary to a reglow occurred, the circuit of the battery which actuated the electromagnet was closed and then opened again ; but there was no sensible brightening as a result of these operations, either at any point of this particular trial or

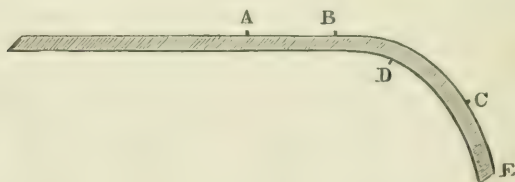
\* The reheating may take place at too low a temperature to produce a reglow.

at any point of any of several subsequent trials which were made. The next consideration was, Was the alternate darkening and reglowing a *subjective* sensation\*? The poker was reheated, and after having been firmly fixed was watched whilst cooling. As soon as the preliminary darkening appeared, the head of the observer was turned slightly and instantly brought back again. The whole operation of turning the head away and back again certainly did not occupy more than a fraction of a second, and yet the metal shone out quite clearly again. Closing and opening the eyes as quickly as possible had a similar effect. No doubt such an instance of subjective sensation is well known to physiologists, but it has given the author a lesson which he will not readily forget respecting the danger of trusting to mere sensation†.

Several other attempts were made with bars and wires of iron and steel; and in the case of certain wires there seemed evidence of more than one real reglow. In face, however, of the warning given by the above experiment, the author prefers to leave the point still open until he has completed some calorimetric observations which he has in view.

*Experiment III.*—A flat iron bar, 60 centim. long, 1·5 centim. broad, and 2 millim. thick, was heated at one end to a very bright red, and observed when cooling in a dark room; a reglow occurred at a very high temperature‡. The bar was again heated, and whilst in the fire was bent as in fig. 2; it

Fig. 2.



was then kept in the fire a short time longer, and afterwards removed. At first all the bar from A to E appeared to be of the same temperature; but presently a delicate cloud appeared on the portion BC, which probably might have escaped notice

\* This was suggested by Mr. Burton at the Meeting of the Physical Society at which the paper was read; the author feels much indebted to Mr. Burton for the suggestion.

† Possibly the intensity of the apparent reglow will be found to differ with different individuals, and even to vary with the state of health at the time. To the author the phenomenon was very striking.

‡ Considerably above dull red.



had it not been for the contrast caused by the unclouded portions AB, CE. Very shortly after its formation the cloud rolled away, and immediately afterwards a reglow occurred throughout the whole of the heated portion of the bar. The eyes had been kept upon the convex side of the bar in this trial; and as it was thought that the concave side might possibly show up bright instead of dark, by contrast with the unstrained metal, the bar was heated a second time and again watched on cooling. Rather contrary to expectation, the portion D, when the cooling had reached a certain point, showed up as being slightly clouded; as before, the cloud rolled away and the reglow immediately succeeded, which also, as before, seemed to raise the whole of the heated portion of the bar to the same temperature. The explanation of the formation of the cloud appears to be this: above but near the critical temperature the specific heat of the strained portion is lower than that of the unstrained portion, and, as a consequence, very shortly after the bar has been removed from the fire the temperature of the former is more rapidly lowered than that of the latter. Again, the reason why the strained portion has a less specific heat than the unstrained, and why, after the reglow, the strained and unstrained portions become apparently once more uniform in temperature, may well be that, in consequence of the greater internal friction of the strained portion, or from some other cause, the contraction of its molecules does not go on at so great a rate. When the temperature of a body is raised, part of the thermal energy expended on it is expended in producing *vis viva* of the molecules, and part in pushing them asunder. If we can prevent the molecules from being pushed asunder, a given amount of thermal energy imparted to the metal will produce a greater rise of temperature. Similarly, when the metal is cooling, a given loss of thermal energy will be attended by a greater lowering of temperature when the contraction of the molecules is lessened. As a consequence, when the strained and unstrained portions are cooling near the critical temperature, there is a greater amount of molecular *potential* energy, and therefore a greater rise of temperature when the explosive change occurs at reglow, in the former than in the latter. The next experiment shows that effects similar to the above can be produced by hammering.

*Experiment IV.*—The straight portion of the bar used in Experiment III. was heated to a white heat, and whilst hot was placed upon a small anvil and struck twice very hard with the sharp end of a hammer at two separate parts, so that two rather deep dents were made in the bar about 10 centim. apart from each other. The bar was now heated a second time to a

very bright red, and was allowed afterwards to cool. When a certain temperature had been reached a very beautiful phenomenon presented itself.

Suppose that AB and CD in fig. 3 represent the two dents.

Fig. 3.



Slight clouds were first seen at AB and CD : these quickly deepened and extended both ways, leaving a bright space at Q. Immediately afterwards the clouds rolled away, beginning from Q, and the whole heated portion brightened with the recalcence. This phenomenon was so striking that the heating and cooling were repeated several times, with apparently no very rapid diminution of the effects of the strain. After the bar had been hammered and bent considerably in this and other experiments of like nature, several attempts were made after straightening it to anneal it ; these attempts were only partially successful. Always before recalcence faint patches of cloud could be detected here and there, showing that some portions were more strained than others. These clouds do not seem to be formed in those specimens of steel or iron which do not show the phenomenon of recalcence ; at least so far as can be judged from observations made with one specimen of steel in which both clouds and perceptible reglow were entirely absent.

*Experiment V.*—Several attempts were made in this experiment to abolish recalcence by shaking or hammering. For this purpose a piece of pianoforte-steel wire, which showed recalcence well, was selected. The wire, which was 1 millim. thick, was suspended vertically with a weight of 1 kilo. on the end. The weight was held by one hand, whilst the other hand was employed in making the wire vibrate transversely whilst cooling ; but no amount of vibrating could prevent the reglow from showing itself. A piece of the same wire was heated to a very bright red in a burner ; it was then removed from the burner, immediately placed on an anvil, and a very sharp blow was given it by a hammer. The blow did not prevent the recalcence from appearing.

In this respect, therefore, the behaviour of iron which has cooled below  $1000^{\circ}$  C. without change of state seems to differ from water which has cooled below  $0^{\circ}$  C. without freezing ; for the latter can be made to freeze by agitation. May not, however, the sudden freezing in this last case be due simply to

the bringing of the liquid water into contact with small crystals of ice which have formed on the side of the vessel. Besides, the internal friction of the iron is very great above the temperature of  $1000^{\circ}\text{C.}$ , and the vibrations may not have been made sufficiently near the point of recalescence\*.

*Experiment VI.*—This experiment was made with a view of ascertaining whether the reglow would occur at a lower temperature the greater the amount of retentivity of the specimen. A specimen of well-annealed Swedish iron wire showed no trace of reglow. This may have been for two reasons:—In the first place, if the reglow occurs very near the critical temperature the very brightness of the wire renders detection difficult; in the second, the amount of potential energy *suddenly* converted into kinetic energy is less. The author does not believe that in this specimen there was no recalescence, for the following reasons:—Whenever recalescence is very marked the sudden changes which have been mentioned as taking place in a wire cooling whilst under stress and strain are also well marked; and, conversely, if there is no sudden change in a wire under stress or strain there is no recalescence: the recalescence and the sudden changes seem to be inseparable companions. Now, with the particular specimen of iron in question, the sudden changes above alluded to could be detected; but they evidently occurred at a much higher temperature than with those specimens of iron and steel which showed recalescence plainly.

As far as could be ascertained from an examination of about a dozen different specimens of iron and steel, those specimens which seemed most capable of being softened by the process of annealing were those in which the phenomenon of recalescence was least marked. When recalescence is manifest, the temperature at which it appears seems to vary considerably with different specimens: with some this temperature would be at least as high as  $800^{\circ}\text{C.}$ , with others at least as low as  $550^{\circ}\text{C.}$

*Views of Professor G. Forbes and Mr. H. F. Newall  
concerning Recalescence.*

According to Professor Forbes †, the phenomenon of recalescence is due to the fact that at a certain temperature there is a sudden increase of the thermal conductivity of iron. As the metal cools from a white heat, the difference between

\* It was impossible to continue the agitations right up to the point of recalescence, as the motion prevented the observer from seeing whether the reglow took place or not.

† Proc. R. S. E. April 6, 1874.

the temperatures of the outside surface and of the inside will after a time become more or less considerable. The outside, being always cooler than the inside, will sooner reach the critical temperature at which there is a sudden increase of thermal conductivity, and, as a consequence, there will be a sudden rush of heat from the inside to the outside; hence the reglow. The assumption that a sudden increase of thermal conductivity should take place when the metal has cooled to a certain temperature is very reasonable; for there is, without doubt, a sudden increase (or, to speak more correctly, more than one\* sudden increase) of electrical conductivity as the iron cools.

Mr. H. F. Newall asserts †, however, that he has shown that the reglow is "not due to differences in conductivity in iron at different temperatures," and that "there is a rise of temperature not only at the surface . . . but also throughout the mass." Mr. Newall has not yet brought before us any experimental evidence in proof of these assertions; but he has promised to do so.

Mr. Newall seems to agree with the author that the process going on during recalescence "partakes of the nature of an explosion, in that once started it continues throughout the mass of the iron," but he regards the rise of temperature as being caused by internal *chemical* action. The author looks forward with much interest to the publication of Mr. Newall's results.

### XIII. *On Cauchy's and Green's Doctrine of Extraneous Force to explain dynamically Fresnel's Kinematics of Double Refraction.* By Sir WILLIAM THOMSON. ‡

1. GREEN'S dynamics of polarization by reflexion, and Stokes' dynamics of the diffraction of polarized light, and Stokes' and Rayleigh's dynamics of the blue sky, all agree in, as seems to me, irrefragably, demonstrating Fresnel's original conclusion, that in plane polarized light the line of vibration is perpendicular to the plane of polarization; the "plane of polarization" being defined as the plane through the ray and perpendicular to the reflecting surface, when light is polarized by reflexion.

\* We should expect, therefore, more than one surface-reheating.

† Phil. Mag. vol. xxiv. No. 150, p. 436 (1887).

‡ Communicated by the Author, having been read before the Royal Society of Edinburgh, Dec. 5, 1887.



2. Now when polarized light is transmitted through a crystal, and when rays in any one of the principal planes are examined, it is found that—

(1) A ray with its plane of polarization in the principal plane travels with the same speed, whatever be its direction (whence it is called the “ordinary ray” for that principal plane); and (2) A ray whose plane of polarization is perpendicular to the principal plane, and which is called “the extraordinary ray” of that plane, is transmitted with velocity differing for different directions, and having its maximum and minimum values in two mutually perpendicular directions of the ray.

3. Hence and by § 1, the velocities of all rays having their vibrations *perpendicular* to one principal plane are the same; and the velocities of rays in a principal plane which have their directions of vibration in the same principal plane, differ according to the direction of the ray, and have maximum and minimum values for directions of the ray at right angles to one another. But in the laminar shearing or distortional motion of which the wave-motion of the light consists, the “plane of the shear”\* (or “plane of the distortion,” as it is sometimes called), is the plane through the direction of the ray and the direction of vibration; and therefore it would be the *ordinary* ray that would have its line of vibration in the principal plane, if the ether’s difference of quality in different directions were merely the aeolotropy of an unstrained elastic solid†. Hence ether in a crystal must have something essentially different from mere intrinsic aeolotropy; something that can give different velocities of propagation to two rays, of one of which the line of vibration and line of propagation coincide respectively with the line of propagation and line of vibration of the other.

4. The difficulty of imagining what this something could possibly be, and the utter failure of dynamics to account for double refraction without it, have been generally felt to be the greatest imperfection of optical theory.

It is true that ever since 1839 a suggested explanation has been before the world; given independently by Cauchy and Green, in what Stokes has called their “Second Theories of Double Refraction,” presented on the same day, the 20th of

\* Thomson and Tait’s ‘Natural Philosophy,’ § 171 (or ‘Elements,’ § 150).

† The elementary dynamics of elastic solids, shows that on this supposition there might be maximum and minimum velocities of propagation for rays in directions at  $45^\circ$  to one another, but that the velocities *must essentially be equal for every two directions at  $90^\circ$  to one another*, in the principal plane, when the line of vibration is in this plane.

May of that year, to the French Academy of Sciences and the Cambridge Philosophical Society. Stokes, in his Report on Double Refraction,\* has given a perfectly clear account of this explanation. It has been but little noticed otherwise, and somehow it has not been found generally acceptable; perhaps, because of a certain appearance of artificiality and arbitrariness of assumption which might be supposed to discredit it. But whatever may have been the reason or reasons which have caused it to be neglected as it has been, and though it is undoubtedly faulty, both as given by Cauchy and by Green, it contains what seems to me, in all probability, the true principle of the explanation, and which is, that the ether in a doubly refracting crystal is an elastic solid, unequally pressed or unequally pulled in different directions, by the unmoved ponderable matter.

5. Cauchy's work on the wave-theory of light is complicated throughout, and to some degree vitiated, by admission of the Navier-Poisson false doctrine† that compressibility is calculable theoretically from rigidity; a doctrine which Green sets aside, rightly and conveniently, by simply assuming incompressibility. In other respects Cauchy's and Green's "Second Theories of Double Refraction" as Stokes calls them, are almost identical. Each supposes ether in the crystal to be an intrinsically aeolotropic elastic solid, having its aeolotropy modified in virtue of internal pressure or pull, equal or unequal in different directions, produced by and balanced by extraneous force. Each is faulty in leaving intrinsic rigidity-modulus (coefficients) unaffected by the equilibrium-pressure, and in introducing three fresh terms, with coefficients (A, B, C in Green's notation) to represent the whole effect of the equilibrium-pressure. This gives for the case of an intrinsically isotropic solid, augmentation of virtual rigidity, and therefore of wave-velocity, by equal pull‡ in all directions, and diminution by equal positive pressure in all directions; which is obviously wrong. Thus definitively, pull in all directions outwards perpendicular to the bounding

\* British Association Report, 1862.

† See Stokes, 'On the Friction of Fluids in Motion and on the Equilibrium and Motion of Elastic Solids,' Camb. Phil. Trans., 1845, §§ 19, 20; reprinted in Stokes 'Mathematical and Physical Papers,' vol. i. p. 123; or Thomson and Tait's 'Natural Philosophy,' §§ 684, 685; or 'Elements,' §§ 655, 656.

‡ So little has been done towards interpreting the formulas of either writer that it has not been hitherto noticed that positive values of Cauchy's G, H, I, or of Green's A, B, C, signify pulls, and negative values signify pressures.

surface equal per unit of area to three times the intrinsic rigidity-modulus, would give quadrupled virtual rigidity, and therefore doubled wave-velocity! Positive normal pressure inwards equal to the intrinsic rigidity-modulus would annul the rigidity and the wave-velocity—that is to say, would make a fluid of the solid. And, on the other hand, negative pressure, or outward pull, on an incompressible liquid, would give it virtual rigidity, and render it capable of transmitting laminar waves! It is obvious that abstract dynamics can show for pressure or pull equal in all directions, no effect on any physical property of an incompressible solid or fluid.

6. Again, pull or pressure *unequal in different directions*, on an isotropic incompressible solid, would, according to Green's formula (A) in p. 303 of his collected Mathematical Papers, cause the velocity of a laminar wave to depend simply on the wave-front, and to have maximum, minimax, and minimum velocities for wave-fronts perpendicular respectively to the directions of maximum pull, minimax pull, and minimum pull; and would make the wave-surface a simple ellipsoid! This, which would be precisely the case of foam stretched unequally in different directions, seemed to me a very interesting and important result, until (as shown in § 19 below) I found it to be not true.

7. To understand fully the stress-theory of double refraction, we may help ourselves effectively by working out directly and thoroughly (as is obviously to be done quite easily by abstract dynamics) the problem of § 6, as follows:—Suppose the solid, isotropic when unstrained, to become strained by pressure so applied to its boundary as to produce, throughout the interior, homogeneous strain according to the following specification:—

The coordinates of any point M of the mass which were  $\xi, \eta, \zeta$  when there was no strain, become in the strained solid

$$\xi\sqrt{\alpha}, \quad \eta\sqrt{\beta}, \quad \zeta\sqrt{\gamma} \quad . \quad . \quad . \quad . \quad . \quad (1);$$

$\sqrt{\alpha}, \sqrt{\beta}, \sqrt{\gamma}$ , or the “Principal Elongations”\*, being the same whatever point M of the solid we choose. Because of incompressibility we have

$$\alpha\beta\gamma = 1 \quad . \quad . \quad . \quad . \quad . \quad (2).$$

\* See chap. iv. of “Mathematical Theory of Elasticity” (W. Thomson), Trans. Roy. Soc. Lond. 1856, reprinted in vol. iii. of ‘Mathematical and Physical Papers,’ now on the point of being published, or Thomson and Tait’s ‘Natural Philosophy,’ §§ 160, 164, or ‘Elements,’ §§ 141, 158.

For brevity, we shall designate as  $(\alpha, \beta, \gamma)$  the strained condition thus defined.

8. As a purely kinematic preliminary, let it be required to find the principal strain-ratios when the solid, already strained according to (1), (2), is further strained by a uniform shear,  $\sigma$ , specified as follows; in terms of  $x, y, z$ , the coordinates of still the same particle,  $M$ , of the solid and other notation, as explained below:—

$$\left. \begin{aligned} x &= \xi\sqrt{\alpha} + \sigma pl \\ y &= \eta\sqrt{\beta} + \sigma pm \\ z &= \zeta\sqrt{\gamma} + \sigma pn \end{aligned} \right\} \dots \dots \dots (3),$$

where

$$p = OP = \lambda\xi\sqrt{\alpha} + \mu\eta\sqrt{\beta} + \nu\zeta\sqrt{\gamma} \dots \dots (4),$$

with

$$l^2 + m^2 + n^2 = 1, \quad \lambda^2 + \mu^2 + \nu^2 = 1 \dots \dots (5),$$

and

$$l\lambda + m\mu + n\nu = 0 \dots \dots \dots (6);$$

$\lambda, \mu, \nu$  denoting the direction-cosines of  $OP$ , the normal to the shearing planes, and  $l, m, n$  the direction-cosines of shearing displacement. The principal axes of the resultant strains are the directions of  $OM$  in which it is maximum or minimum, subject to the condition

$$\xi^2 + \eta^2 + \zeta^2 = 1 \dots \dots \dots (7);$$

and its maximum, minimax, and minimum values are the three required strain-ratios. Now we have

$$\begin{aligned} OM^2 &= x^2 + y^2 + z^2 \\ &= \xi^2\alpha + \eta^2\beta + \zeta^2\gamma + 2\sigma(l\xi\sqrt{\alpha} + m\eta\sqrt{\beta} + n\zeta\sqrt{\gamma})p + \sigma^2p^2 \end{aligned} \quad (8),$$

and to make this maximum or minimum subject to (7), we have

$$\frac{d(\frac{1}{2}OM^2)}{d\xi} = \rho\xi; \quad \frac{d(\frac{1}{2}OM^2)}{d\eta} = \rho\eta; \quad \frac{d(\frac{1}{2}OM^2)}{d\zeta} = \rho\zeta \dots \dots (9);$$

where in virtue of (7), and because  $OM^2$  is a homogeneous quadratic function of  $\xi, \eta, \zeta$ ,

$$\rho = OM^2.$$

The determinantal cubic, being

$$(\mathcal{A} - \rho)(\mathcal{B} - \rho)(\mathcal{C} - \rho) - a^2(\mathcal{A} - \rho) - b^2(\mathcal{B} - \rho) - c^2(\mathcal{C} - \rho) + 2abc = 0 \quad (10),$$

where

$$\begin{aligned} \mathcal{A} &= \alpha(1 + 2\sigma l\lambda + \sigma^2\lambda^2); \quad \mathcal{B} = \beta(1 + 2\sigma m\mu + \sigma^2\mu^2); \\ \mathcal{C} &= \gamma(1 + 2\sigma n\nu + \sigma^2\nu^2) \dots \dots \dots (11) \end{aligned}$$



and

$$a = \sqrt{(\beta\gamma)[\sigma(m\nu + n\mu) + \sigma^2\mu\nu]}; \quad b = \sqrt{(\gamma\alpha)[\sigma(n\lambda + l\nu) + \sigma^2\nu\lambda]}; \\ c = \sqrt{(\alpha\beta)[\sigma(l\mu + m\lambda) + \sigma^2\lambda\mu]} \quad . \quad . \quad . \quad (12),$$

gives three real positive values for  $\rho$ , the square roots of which are the required principal strain-ratios.

9. Entering now on the dynamics of our subject, remark that the isotropy (§ 1), implies that the work required of the extraneous pressure, to change the solid from its unstrained condition (1, 1, 1) to the strain  $(\alpha, \beta, \gamma)$ , is independent of the direction of the normal axes of the strain, and depends solely on the magnitudes of  $\alpha, \beta, \gamma$ . Hence if  $E$  denotes its magnitude per unit of volume; or the potential energy of unit volume in the condition  $(\alpha, \beta, \gamma)$  reckoned from zero in the condition (1, 1, 1); we have

$$E = \psi(\alpha, \beta, \gamma) \quad . \quad . \quad . \quad . \quad . \quad . \quad (13),$$

where  $\psi$  denotes a function of which the magnitude is unaltered when the values of  $\alpha, \beta, \gamma$  are interchanged. Consider a portion of the solid, which, in the unstrained condition, is a cube of unit side, and which in the strained condition  $(\alpha, \beta, \gamma)$ , is a rectangular parallelepiped  $\sqrt{\alpha} \cdot \sqrt{\beta} \cdot \sqrt{\gamma}$ . In virtue of isotropy and symmetry, we see that the pull or pressure on each of the six faces of this figure, required to keep the substance in the condition  $(\alpha, \beta, \gamma)$  is normal to the face. Let the amounts of these forces per unit area, on the three pairs of faces respectively, be  $A, B, C$ , each reckoned as positive or negative according as the force is positive *pull*, or positive pressure. We shall take

$$A + B + C = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad (14),$$

because normal pull or pressure uniform in all directions produces no effect, the solid being incompressible. The work done on any infinitesimal change from the configuration  $(\alpha, \beta, \gamma)$  is

$$\left. \begin{aligned} & A \sqrt{(\beta\gamma)} d(\sqrt{\alpha}) + B \sqrt{(\gamma\alpha)} d(\sqrt{\beta}) + C \sqrt{(\alpha\beta)} d(\sqrt{\gamma}), \\ & \text{or (because } \alpha\beta\gamma=1) \\ & \frac{A}{2\alpha} d\alpha + \frac{B}{2\beta} d\beta + \frac{C}{2\gamma} d\gamma \end{aligned} \right\} (15).$$

10. Let  $\delta\alpha, \delta\beta, \delta\gamma$  be any variations of  $\alpha, \beta, \gamma$  consistent with (2), so that we have

$$\left. \begin{aligned} & (\alpha + \delta\alpha)(\beta + \delta\beta)(\gamma + \delta\gamma) = 1 \\ & \alpha\beta\gamma = 1 \end{aligned} \right\} \quad . \quad . \quad (16).$$

Now suppose  $\delta\alpha, \delta\beta, \delta\gamma$  to be so small that we may neglect

their cubes and corresponding products, and all higher products. We have

$$\frac{\delta\alpha}{\alpha} + \frac{\delta\beta}{\beta} + \frac{\delta\gamma}{\gamma} + \alpha\delta\beta\delta\gamma + \beta\delta\gamma\delta\alpha + \gamma\delta\alpha\delta\beta = 0 \quad (17);$$

whence

$$\left(\frac{\delta\alpha}{\alpha}\right)^2 = \left(\frac{\delta\beta}{\beta} + \frac{\delta\gamma}{\gamma}\right)^2;$$

whence, and by the symmetrical expressions,

$$\left. \begin{aligned} 2\delta\beta\delta\gamma &= \frac{1}{\alpha} \left( \frac{\delta\alpha^2}{\alpha^2} - \frac{\delta\beta^2}{\beta^2} - \frac{\delta\gamma^2}{\gamma^2} \right) \\ 2\delta\gamma\delta\alpha &= \frac{1}{\beta} \left( \frac{\delta\beta^2}{\beta^2} - \frac{\gamma^2}{\gamma^2} - \frac{\delta\alpha^2}{\alpha^2} \right) \\ 2\delta\alpha\delta\beta &= \frac{1}{\gamma} \left( \frac{\delta\gamma^2}{\gamma^2} - \frac{\delta\alpha^2}{\alpha^2} - \frac{\delta\beta^2}{\beta^2} \right) \end{aligned} \right\} \quad \dots \quad (18).$$

11. Now, if  $E + \delta E$  denote the energy per unit bulk of the solid in the condition

$$(\alpha + \delta\alpha, \beta + \delta\beta, \gamma + \delta\gamma),$$

we have, by Taylor's theorem,

$$\delta E = H_1 + H_2 + H_3 + \&c.,$$

where  $H_1, H_2, \&c.$  denote homogeneous functions of  $\delta\alpha, \delta\beta, \delta\gamma$  of the 1st degree, 2nd degree, &c. Hence, omitting cubes &c., and eliminating the products from  $H_2$ , and taking  $H_1$  from (15), we find

$$\delta E = \frac{1}{2} \left( \frac{A}{\alpha} \delta\alpha + \frac{B}{\beta} \delta\beta + \frac{C}{\gamma} \delta\gamma + G \frac{\delta\alpha^2}{\alpha^2} + H \frac{\delta\beta^2}{\beta^2} + I \frac{\delta\gamma^2}{\gamma^2} \right). \quad (19),$$

where  $G, H, I$  denote three coefficients depending on the nature of the function  $\psi$  (13), which expresses the energy. Thus in (19), with (14) taken into account, we have just five coefficients independently disposable,  $A, B, G, H, I$ , which is the right number because, in virtue of  $\alpha\beta\gamma=1$ ,  $E$  is a function of just two independent variables.

12. For the case of  $\alpha=1, \beta=1, \gamma=1$ , we have

$$A=B=C=0 \quad \text{and} \quad G=H=I=G_1, \text{ suppose;}$$

which give

$$\delta E = \frac{1}{2} G_1 (\delta\alpha^2 + \delta\beta^2 + \delta\gamma^2).$$

From this we see that  $2G_1$  is simply the rigidity-modulus of the unstrained solid; because if we make  $\delta\gamma=0$ , we have  $\delta\alpha=-\delta\beta$ , and the strain becomes an infinitesimal distortion

in the plane (*xy*), which may be regarded in two ways as a simple shear of which the magnitude is  $\delta\alpha^*$  (this being twice the elongation in one of the normal axes).

13. Going back to (10), (11), and (12), let  $\sigma$  be so small that  $\sigma^3$  and higher powers can be neglected. To this degree of approximation we neglect  $abc$  in (10), and see that its three roots are respectively

$$\mathcal{A} = \frac{b^2}{c - \mathcal{A}} - \frac{c^2}{\mathcal{B} - \mathcal{A}}; \quad \mathcal{B} = \frac{c^2}{\mathcal{A} - \mathcal{B}} - \frac{a^2}{c - \mathcal{B}}; \quad \mathcal{C} = \frac{a^2}{\mathcal{B} - \mathcal{C}} - \frac{b^2}{\mathcal{A} - \mathcal{C}} \quad (20),$$

provided none of the differences constituting the denominators is infinitely small. The case of any of these differences infinitely small, or zero, does not, as we shall see in the conclusion, require special treatment, though special treatment would be needed to interpret for any such case each step of the process.

14. Substituting now for  $\mathcal{A}$ ,  $\mathcal{B}$ ,  $\mathcal{C}$ ,  $a$ ,  $b$ ,  $c$  in (20), their values by (11) and (12), neglecting  $\sigma^3$  and higher powers, and denoting by  $\delta\alpha$ ,  $\delta\beta$ ,  $\delta\gamma$  the excesses of the three roots above  $\alpha$ ,  $\beta$ ,  $\gamma$  respectively, we find

$$\left. \begin{aligned} \delta\alpha &= \alpha \left\{ 2\sigma/\lambda + \sigma^2 \left[ \lambda^2 - \frac{\gamma}{\gamma - \alpha} (n\lambda + l\nu)^2 - \frac{\beta}{\beta - \alpha} (l\mu + m\lambda)^2 \right] \right\} \\ \delta\beta &= \beta \left\{ 2\sigma m\mu + \sigma^2 \left[ \mu^2 - \frac{\alpha}{\alpha - \beta} (l\mu + m\lambda)^2 - \frac{\gamma}{\gamma - \beta} (m\nu + n\mu)^2 \right] \right\} \\ \delta\gamma &= \gamma \left\{ 2\sigma n\nu + \sigma^2 \left[ \nu^2 - \frac{\beta}{\beta - \gamma} (m\nu + n\mu)^2 - \frac{\alpha}{\alpha - \gamma} (n\lambda + l\nu)^2 \right] \right\} \end{aligned} \right\} \quad (21),$$

and using these in (19), we find

$$\left. \begin{aligned} \delta E &= \sigma(A l \lambda + B m \mu + C n \nu) \\ &+ \frac{1}{2} \sigma^2 \{ A \lambda^2 + B \mu^2 + C \nu^2 + L(m\nu + n\mu)^2 + M(n\lambda + l\nu)^2 + N(l\mu + m\lambda)^2 \} \\ &+ 2\sigma^2 (G l^2 \lambda^2 + H m^2 \mu^2 + I n^2 \nu^2) \end{aligned} \right\} \quad (22),$$

where

$$L = \frac{B\gamma - C\beta}{\beta - \gamma}; \quad M = \frac{C\alpha - A\gamma}{\gamma - \alpha}; \quad N = \frac{A\beta - B\alpha}{\alpha - \beta}. \quad (23)$$

15. Now from (5) and (6) we find

$$(m\nu + n\mu)^2 = 1 - l^2 - \lambda^2 + 2(l^2 \lambda^2 - m^2 \mu^2 - n^2 \nu^2) \quad (24),$$

which, with the symmetrical expressions, reduces (22) to

\* Thomson and Tait's 'Natural Philosophy,' § 175, or 'Elements,' § 154.

$$\begin{aligned} E = & \sigma(A\lambda + Bm\mu + Cnv) + \frac{1}{2}\sigma^2\{L + M + N + (A - L)\lambda^2 \\ & + (B - M)\mu^2 + (C - N)v^2 - Ll^2 - Mm^2 - Nn^2 + 2[(2G + L - M - N)l^2\lambda^2 \\ & + (2H + M - N - L)m^2\mu^2 + (2I + N - L - M)n^2v^2]\} \end{aligned} \quad (25).$$

16. To interpret this result statically, imagine the solid to be given in the state of homogeneous strain ( $\alpha$ ,  $\beta$ ,  $\gamma$ ) throughout, and let a finite plane plate of it, of thickness  $h$ , and of very large area  $Q$ , be displaced by a shearing motion according to the specification (3), (4), (5), (6) of § 8; the bounding-planes of the plate being unmoved, and all the solid exterior to the plate being therefore undisturbed except by the slight distortion round the edge of the plate produced by the displacement of its substance. The analytical expression of this is

$$\sigma = f(p) \quad . \quad . \quad . \quad . \quad . \quad (26),$$

where  $f$  denotes any function of OP such that

$$\int_0^h dp f(p) = 0 \quad . \quad . \quad . \quad . \quad . \quad (27).$$

If we denote by  $W$  the work required to produce the supposed displacement, we have

$$W = Q \int_0^h dp \delta E + \mathcal{U} \quad . \quad . \quad . \quad . \quad (28),$$

$\delta E$  being given by (25), with everything constant except  $\sigma$ , a function of OP; and  $\mathcal{U}$  denoting the work done on the solid outside the boundary of the plate. In this expression the first line of (25) disappears in virtue of (27); and we have

$$\left. \begin{aligned} \frac{W - \mathcal{U}}{Q} = & \frac{1}{2}\{L + M + N + (A - L)\lambda^2 + (B - M)\mu^2 + (C - N)v^2 \\ & - Ll^2 - Mm^2 - Nn^2 + 2[(2G + L - M - N)l^2\lambda^2 \\ & + (2H + M - N - L)m^2\mu^2 + (2I + N - L - M)n^2v^2]\} \int_0^h dp \sigma^2 \end{aligned} \right\} \quad (29).$$

When every diameter of the plate is infinitely great in comparison with its thickness,  $\mathcal{U}/Q$  is infinitely small; and the second member of (29) expresses the work per unit of area of the plate, required to produce the supposed shearing motion.

17. Solve now the problem of finding, subject to (5) and (6) of § 8, the values of  $l$ ,  $m$ ,  $n$  which make the factor  $\int_0^h dp \sigma^2$  of the second member of (29) a maximum or minimum. This is only the problem of finding the two principal diameters of the ellipse in which the ellipsoid



$$[2(2G + L - M - N)\lambda^2 - L]x^2 + [2(2H + M - N - L)\mu^2 - M]y^2 + [2(2I + N - L - M)\nu^2 - N]z^2 = \text{const.} \quad (30)$$

is cut by the plane

$$\lambda x + \mu y + \nu z = 0 \quad (31).$$

If the displacement is in either of the two directions ( $l, m, n$ ) thus determined, the force required to maintain it is in the direction of displacement; and the magnitude of this force per unit bulk of the material of the plate at any point within it is easily proved to be

$$\{M\} \frac{d\sigma}{d\rho} \quad (32),$$

where  $\{M\}$  denotes the maximum or the minimum value of the bracketed factor of (29).

18. Passing now from equilibrium to motion, we see at once that (the density being taken as unity)

$$V^2 = \{M\} \quad (33),$$

where  $V$  denotes the velocity of either of two simple waves whose wave-front is perpendicular to  $(\lambda, \mu, \nu)$ . Consider the case of wave-front perpendicular to one of the three principal planes; ( $yz$ ) for instance: we have  $\lambda = 0$ ; and, to make  $\{ \}$  of (29) a maximum or minimum, we see by symmetry that we must either have

$$\left. \begin{array}{l} \text{(vibration perpendicular to principal plane)} \quad l=1, m=0, n=0 \\ \text{(vibration in principal plane)} \quad \dots\dots\dots l=0, m=-\nu, n=\mu \end{array} \right\} \quad (34)$$

Hence, for the two cases, we have respectively:

$$\text{Vibration perpendicular to } yz \quad \dots \quad V^2 = M + N + (B - M)\mu^2 + (C - N)\nu^2 \quad (35)$$

$$\text{Vibration in } yz \quad \dots\dots\dots V^2 = L + B\mu^2 + C\nu^2 + 4(H + I - L)\mu^2\nu^2 \quad (36)$$

19. According to Fresnel's theory (35) must be constant, and the last term of (36) must vanish. These and the corresponding conclusions relatively to the other two principal planes are satisfied if, and require that,

$$A - L = B - M = C - N \quad (37),$$

and

$$H + I = L; \quad I + G = M; \quad G + H = N \quad (38).$$

Transposing  $M$  and  $N$  in the last of equations (37), substituting for them their values by (23), and dividing each member by

$\beta\gamma$ , we find

$$\frac{C-A}{\beta\gamma-\alpha\beta} = \frac{A-B}{\gamma\alpha-\beta\gamma} \quad \dots \quad (39);$$

whence (sum of numerators divided by sum of denominators),

$$\frac{B-C}{\gamma\alpha-\alpha\beta} = \frac{C-A}{\alpha\beta-\beta\gamma} = \frac{A-B}{\beta\gamma-\gamma\alpha} \quad \dots \quad (40).$$

The first of these equations is equivalent to the first of (37); and thus we see that the two equations (37) are equivalent to one only; and (39) is a convenient form of this one. By it, as put symmetrically in (40), and by bringing (14) into account, we find, with  $k$  taken to denote a coefficient which may be any function of  $(\alpha, \beta, \gamma)$ :

$$\left. \begin{aligned} A &= k(S - \beta\gamma); & B &= k(S - \gamma\alpha); & C &= k(S - \alpha\beta); \end{aligned} \right\} \quad (41):$$

where  $S = \frac{1}{3}(\beta\gamma + \gamma\alpha + \alpha\beta)$

and using this result in (23), we find

$$\left. \begin{aligned} L &= k[\alpha(\beta + \gamma) - S]; & M &= k[\beta(\gamma + \alpha) - S]; & N &= k[\gamma(\alpha + \beta) - S] \end{aligned} \right\} \quad (42).$$

or  $L = k(2S - \beta\gamma); M = k(2S - \gamma\alpha); N = k(2S - \alpha\beta)$

By (2) we may put (41) and (42) into forms more convenient for some purposes as follows:—

$$A = k\left(S - \frac{1}{\alpha}\right); \quad B = k\left(S - \frac{1}{\beta}\right); \quad C = k\left(S - \frac{1}{\gamma}\right) \quad (43),$$

$$L = k\left(2S - \frac{1}{\alpha}\right); \quad M = k\left(2S - \frac{1}{\beta}\right); \quad N = k\left(2S - \frac{1}{\gamma}\right) \quad (44),$$

where

$$S = \frac{1}{3}\left(\frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma}\right) \quad \dots \quad (45).$$

Next, we find  $G, H, I$ ; by (38), (44), and (45) we have

$$G + H + I = \frac{1}{2}(L + M + N) = \frac{3}{2}kS = \frac{1}{2}k\left(\frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma}\right) \quad (46),$$

whence, by (38) and (44),

$$G = k\left(\frac{1}{\alpha} - \frac{1}{2}S\right); \quad H = k\left(\frac{1}{\beta} - \frac{1}{2}S\right); \quad I = k\left(\frac{1}{\gamma} - \frac{1}{2}S\right) \quad (47).$$

20. Using (43) and (47) in (19), we have

$$\delta E = \frac{1}{2}k \left\{ -\frac{\delta\alpha}{\alpha^2} - \frac{\delta\beta}{\beta^2} - \frac{\delta\gamma}{\gamma^2} + S\left(\frac{\delta\alpha}{\alpha} + \frac{\delta\beta}{\beta} + \frac{\delta\gamma}{\gamma}\right) \right. \\ \left. + \frac{\delta\alpha^2}{\alpha^3} + \frac{\delta\beta^2}{\beta^3} + \frac{\delta\gamma^2}{\gamma^3} - \frac{1}{2}S\left(\frac{\delta\alpha^2}{\alpha^2} + \frac{\delta\beta^2}{\beta^2} + \frac{\delta\gamma^2}{\gamma^2}\right) \right\} \quad (48)$$

Now we have, by (2),  $\log(\alpha\beta\gamma)=0$ . Hence, taking the variation of this as far as terms of the second order,

$$\frac{\delta\alpha}{\alpha} + \frac{\delta\beta}{\beta} + \frac{\delta\gamma}{\gamma} - \frac{1}{2} \left( \frac{\delta\alpha^2}{\alpha^2} + \frac{\delta\beta^2}{\beta^2} + \frac{\delta\gamma^2}{\gamma^2} \right) = 0 \quad (49);$$

which reduces (48) to

$$\delta E = \frac{1}{2} k \left( -\frac{\delta\alpha}{\alpha^2} - \frac{\delta\beta}{\beta^2} - \frac{\delta\gamma}{\gamma^2} + \frac{\delta\alpha^2}{\alpha^3} + \frac{\delta\beta^2}{\beta^3} + \frac{\delta\gamma^2}{\gamma^3} \right) \quad (50).$$

Remembering that cubes and higher powers are to be neglected, we see that (50) is equivalent to

$$\delta E = \frac{1}{2} k \delta \left( \frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma} \right) \quad (51).$$

Hence if we take  $k$  constant, we have

$$E = \frac{1}{2} k \left( \frac{1}{\alpha} + \frac{1}{\beta} + \frac{1}{\gamma} - 3 \right) \quad (52);$$

and it is clear that  $k$  must be stationary (that is to say,  $\delta k=0$ ) for any particular values of  $\alpha, \beta, \gamma$  for which (51) holds; and if (51) holds for all values,  $k$  must be constant for all values of  $\alpha, \beta, \gamma$ .

21. Going back to (29), taking  $Q$  great enough to allow  $\mathcal{U}/Q$  to be neglected, and simplifying by (46), (43), and (44), we find

$$\frac{W}{Q} = k \left( \frac{l^2}{\alpha} + \frac{m^2}{\beta} + \frac{n^2}{\gamma} \right) \int_0^h dp \sigma^2 \quad (53);$$

and the problem (§ 17) of determining  $l, m, n$ , subject to (5) and (6), to make  $l^2/\alpha + m^2/\beta + n^2/\gamma$  a maximum or minimum for given values of  $\lambda, \mu, \nu$ , yields the equations

$$\omega\lambda - \omega'l + \frac{l}{\alpha} = 0; \quad \omega\mu - \omega'm + \frac{m}{\beta} = 0; \quad \omega\nu - \omega'n + \frac{n}{\gamma} = 0. \quad (54),$$

$\omega, \omega'$  denoting indeterminate multipliers; whence

$$\omega' = \frac{l^2}{\alpha} + \frac{m^2}{\beta} + \frac{n^2}{\gamma} \quad (55),$$

$$\omega^2 = l^2 \left( \omega' - \frac{1}{\alpha} \right)^2 + m^2 \left( \omega' - \frac{1}{\beta} \right)^2 + n^2 \left( \omega' - \frac{1}{\gamma} \right)^2 \quad (56),$$

$$\left. \begin{aligned} \omega\lambda &= l \left( -\frac{1-l^2}{\alpha} + \frac{m^2}{\beta} + \frac{n^2}{\gamma} \right), \\ \omega\mu &= m \left( \frac{l^2}{\alpha} - \frac{1-m^2}{\beta} + \frac{n^2}{\gamma} \right), \\ \omega\nu &= n \left( \frac{l^2}{\alpha} + \frac{m^2}{\beta} - \frac{1-n^2}{\gamma} \right), \end{aligned} \right\} \quad (57).$$

These formulas are not directly convenient for finding  $l, m, n$  from  $\lambda, \mu, \nu$  given (the ordinary formulas for doing so need not be written here); but they give  $\lambda, \mu, \nu$  explicitly in terms of  $l, m, n$  supposed known; that is to say, they solve the problem of finding the wave-front of the simple laminar wave whose direction of vibration is  $(l, m, n)$ . The velocity is given by

$$v^2 = k \left( \frac{l^2}{\alpha} + \frac{m^2}{\beta} + \frac{n^2}{\gamma} \right) \dots \dots \dots (58).$$

It is interesting to notice that this depends solely on the direction of the line of vibration; and that (except in special cases, of partial or complete isotropy) there is just one wave-front for any given line of vibration. These are precisely in every detail the conditions of Fresnel's Kinematics of Double Refraction.

22. Going back to (35) and (36), let us see if we can fit them to double refraction with line of vibration *in* the plane of polarization. This would require (36) to be the ordinary ray, and therefore requires the fulfilment of (38), as did the other supposition; but instead of (37) we now have [in order to make (36) constant]

$$A = B = C \dots \dots \dots (59),$$

and therefore each, in virtue of (14), zero; and

$$\alpha = \beta = \gamma = 1;$$

so that we are driven to complete isotropy. Hence our present form (§ 7) of the stress theory of double refraction *cannot* be fitted to give line of vibration *in* the plane of polarization. We have seen (§ 21) that it *does* give line of vibration *perpendicular to the plane of polarization with exactly Fresnel's form* of wave-surface, when fitted for the purpose, by the simple assumption that the potential energy of the strained solid is expressed by (52) with  $k$  constant! It is important to remark that  $k$  is the rigidity-modulus of the unstrained isotropic solid.

23. From (58) we see that the velocities of the waves corresponding to the three cases,  $l=1, m=1, n=1$ , respectively are  $\sqrt{k/\alpha}$ ,  $\sqrt{k/\beta}$ ,  $\sqrt{k/\gamma}$ . Hence the velocity of any wave whose vibrations are parallel to any one of the three principal elongations, multiplied by this elongation, is equal to the velocity of a wave in the unstrained isotropic solid.



XIV. *On the Diffusion of Gases; a Reply to Professor Tait.*  
By S. H. BURBURY\*.

SUPPOSE a gas at uniform density in a closed vessel, and to the vessel and every molecule in it a small common velocity  $u$  be given. Then the gas has *mass motion*  $u$ .

In the problem of diffusion the gas has density  $n_1$  independent of the time but varying from point to point along a tube; and another gas has density  $n_2$  varying in the opposite way, so that  $n_1 + n_2$  is constant. The first gas acquires a steady stream, say from left to right, along the tube. And the question is whether the steady motion of the gas in such circumstances can be "mass motion" as above defined, or not.

I maintain that it cannot; and, unless by the weight of his authority, Professor Tait has not shaken my reasoning.

Take an element of the tube, of length  $\delta x$ , between the sections A and A'. Without departing from the statistical method, I say there exists a class  $v$  of molecules of the first gas in that element constant in number, namely  $n_1 f(v) dv \delta x$ , while the particular molecules composing it are continually changing. Without departing from the statistical method, I say there are two, and only two, possible ways in which they can change—(1) by molecules with velocity  $v$  passing in or out at A and A', (2) by encounters within the element. As the result of (1), molecules going to the right enter the element in greater numbers than they leave it. The reverse is the case with those going to the left. From this cause you have an increase of translation-velocity of the class within the element equal to  $f(v) dv \frac{v^2}{3} \frac{dn_2}{dx} \delta x$  per unit of time.

(2) By encounters  $n_1 f(v) dv B \delta x$  molecules leave the class, and as many enter it, per unit of time. The original molecules had mean translation-velocity  $\alpha$ . The substituted ones have mean translation-velocity  $\alpha'$ , which is less than  $\alpha$ .

Owing to encounters, you have a loss of translation-velocity to the class within the element equal to  $n_1 f(v) dv B (\alpha - \alpha') \delta x$  per unit time. Equating the loss by (2) to the gain by (1), you obtain the condition for steady motion,

$$\frac{v^2}{3} \frac{dn_2}{dx} = n_1 B (\alpha - \alpha'),$$

for each class.

It cannot be satisfied by "mass motion." I have not "ignored the community of interest which mutual collisions

\* Communicated by the Author.

secure for all the particles of a gas." Mutual collisions secure the continual change of the individual particles constituting a particular class, and the diminution of  $\alpha$ , and that is all the influence they have in the case under consideration.

I find, however, that in one respect I misunderstood Professor Tait. I said that on his hypothesis  $\alpha$ , the translation-velocity of the class  $r$  in the moving gas, would be constant. But that would not be the effect of mass motion. If we give to a gas, previously at rest, mass motion  $u$ , the number of molecules in the gas so moving whose velocities are between  $r$  and  $r+dr$ , and make with the direction  $u$  angles between  $\psi$  and  $\psi+d\psi$ , is

$$C\epsilon^{-hv^2}e^2 dr(1+2huv \cos \psi) \frac{\sin \psi d\psi}{2};$$

and therefore the mean translation-velocity of the class  $r$  is, not  $u$ , but  $\frac{2}{3}hr^2u$ . In order to represent Professor Tait's hypothesis I should make in my notation  $\alpha = \frac{2}{3}hr^2\bar{\alpha}$ . But it would not satisfy the condition for steady motion.

XV. *On Electromagnetic Waves, especially in relation to the Vorticity of the Impressed Forces; and the Forced Vibrations of Electromagnetic Systems.* By OLIVER HEAVISIDE\*.

1. **SUMMARY** of *Electromagnetic Connexions*.—To avoid indistinctness, I start with a short summary of Maxwell's scheme, so far as its essentials are concerned, in the form given by me in January 1885†.

Two forces, electric and magnetic,  $\mathbf{E}$  and  $\mathbf{H}$ , connected with the three fluxes,—electric displacement  $\mathbf{D}$ , conduction-current  $\mathbf{C}$ , and magnetic induction  $\mathbf{B}$ ; thus

$$\mathbf{B} = \mu\mathbf{H}, \quad \mathbf{C} = k\mathbf{E}, \quad \mathbf{D} = (c/4\pi)\mathbf{E}. \quad . \quad . \quad . \quad (1)$$

Two currents, electric and magnetic,  $\mathbf{F}$  and  $\mathbf{G}$ , each of which is proportional to the curl or vorticity of the *other* force, not counting impressed; thus,

$$\text{curl}(\mathbf{H} - \mathbf{h}) = 4\pi\mathbf{F}, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\text{curl}(\mathbf{e} - \mathbf{E}) = 4\pi\mathbf{G}; \quad . \quad . \quad . \quad . \quad . \quad (3)$$

where  $\mathbf{e}$  and  $\mathbf{h}$  are the impressed parts of  $\mathbf{E}$  and  $\mathbf{H}$ . These currents are also directly connected with the corresponding forces through

$$\mathbf{F} = \mathbf{C} + \dot{\mathbf{D}}, \quad \mathbf{G} = \dot{\mathbf{B}}/4\pi. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

\* Communicated by the Author.

† See the opening sections of "Electromagnetic Induction and its Propagation," *Electrician*, Jan. 3, 1885, and after.



2. *Plane Sheets of Impressed Force in a Nonconducting Dielectric.*—We need only refer to impressed electric force  $\mathbf{e}$ , as solutions relating to  $\mathbf{h}$  are quite similar. Let an infinitely extended nonconducting dielectric be divided into two regions by an infinitely extended plane  $(x, y)$ , on one side of which, say the left, or that of  $-z$ , is a field of  $\mathbf{e}$  of uniform intensity  $e$ , but varying with the time. If it be perpendicular to the boundary, it produces no flux. Only the tangential component can be operative. Hence we may suppose that  $\mathbf{e}$  is parallel to the plane, and choose it parallel to  $x$ . Then  $\mathbf{E}$ , the force of the flux, is parallel to  $x$ , of intensity  $\mathbf{E}$  say, and the magnetic force, of intensity  $\mathbf{H}$ , is parallel to  $y$ . Let  $e=f(t)$ ; the complete solutions due to the impressed force are then

$$\mathbf{E} = \mu v \mathbf{H} = -\frac{1}{2}f(t - z/v) \quad . \quad . \quad . \quad (9)$$

on the right side of the plane, where  $z$  is  $+$ , and

$$-\mathbf{E} = \mu v \mathbf{H} = -\frac{1}{2}f(t + z/v) \quad . \quad . \quad . \quad (10)$$

on the left side of the plane, where  $z$  is  $-$ . In the latter case we must deduct the impressed force from  $\mathbf{E}$  to obtain the force of the field, say  $\mathbf{F}$ , which is therefore

$$\mathbf{F} = -f(t) + \frac{1}{2}f\left(t + \frac{z}{v}\right) \quad . \quad . \quad . \quad (11)$$

The results are most easily followed thus. At the plane itself, where the vortex-lines of  $\mathbf{e}$  are situated, we, by varying  $e$ , produce simultaneous changes in  $\mathbf{H}$ , thus,

$$\mathbf{H} = \frac{e}{2\mu v}, \quad . \quad . \quad . \quad (12)$$

at the plane. This disturbance is then propagated both ways undistorted at the speed  $v = (\mu c)^{-\frac{1}{2}}$ .

On the other hand, the corresponding electric displacements are oppositely directed on the two sides of the plane.

Since the line-integral of  $\mathbf{H}$  is electric current, and the line-integral of  $e$  is electromotive force, the ratio of  $e$  to  $\mathbf{H}$  is the resistance-operator of an infinitely long tube of unit area; a constant, measurable in ohms, being 60 ohms in vacuum, or 30 ohms on each side. Why it is a constant is simply

they may be added on), and allows us to work without change of notation, especially when the vectors are in special type, as they should be, being entities of widely different nature from scalars. I denote a vector by (say)  $\mathbf{E}$ , its tensor by  $\mathbf{E}$ , and its  $x, y, z$  components, when wanted, by  $E_x, E_y, E_z$ . The perpetually occurring scalar product of two vectors requires no prefix. The prefix  $\mathbf{V}$  of a vector product should be a special symbol.



because the waves cannot return, as there is no reflecting barrier in the infinite dielectric.

3. If the impressed force be confined to the region between two parallel planes distant  $2a$  from one another, there are now two sources of disturbances, which are of opposite natures, because the vorticity of  $\mathbf{e}$  is oppositely directed on the two planes, so that the left plane sends out both ways disturbances which are the negatives of those simultaneously emitted by the right plane. Thus, if the origin of  $z$  be midway between the planes, we shall have

$$E = \mu v H = -\frac{1}{2}f\left(t - \frac{z-a}{v}\right) + \frac{1}{2}f\left(t - \frac{z+a}{v}\right) \quad . \quad (13)$$

on the right side of the stratum of  $e$ , and

$$-E = \mu v H = -\frac{1}{2}f\left(t + \frac{z+a}{v}\right) + \frac{1}{2}f\left(t + \frac{z-a}{v}\right) \quad . \quad (14)$$

on the left side. If therefore  $e$  vary periodically in such a way that

$$f(t) = -f(t+2a), \quad . \quad . \quad . \quad . \quad (15)$$

there is no disturbance outside the stratum, after the initial waves have gone off, the disturbance being then confined to the stratum of impressed force.

Decreasing the thickness of the stratum indefinitely leads to the result that the effect due to  $e=f(t)$  in a layer of thickness  $dz$  at  $z=0$  is, on the right side,

$$\begin{aligned} H &= -\frac{1}{2\mu v} \left\{ f\left(t - \frac{z}{v}\right) - f\left(t - \frac{z+dz}{v}\right) \right\} \\ &= -\frac{c}{2} \frac{dz}{v} f'\left(t - \frac{z}{v}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (16) \end{aligned}$$

since  $\mu cv^2 = 1$ ; on the left side the  $+$  sign is required.

We can now, by integration, express the effect due to  $e=f(z, t)$ , viz.

$$H = -\frac{c}{2} \int_{-\infty}^z \frac{d}{dt} f\left(t - \frac{z-z'}{v}, z'\right) dz' + \frac{c}{2} \int_z^{\infty} \frac{d}{dt} \left(t + \frac{z-z'}{v}, z'\right) dz', \quad (17)$$

$$E = e - \frac{1}{2v} \int_{-\infty}^z \frac{d}{dt} f\left(t - \frac{z-z'}{v}, z'\right) dz' - \frac{1}{2v} \int_z^{\infty} \frac{d}{dt} \left(t + \frac{z-z'}{v}, z'\right) dz'. \quad (18)$$

In these, however, a certain assumption is involved, viz. that  $e$  vanishes at  $\infty$  both ways, because we base the formulæ upon (16), which concerns a layer of  $e$  on both sides of which  $e$  is zero. Now the disturbances really depend upon  $de/dz$ , for there can be none if this be zero. By (12) the elementary

$de/dz$  through distance  $dz$  instantly produces

$$H = \frac{1}{2\mu v} \frac{de}{dz} dz \quad . \quad . \quad . \quad . \quad . \quad (19)$$

at the place. If, therefore,  $e=f(z, t)$ , the  $H$  solution at any point consists of the positive waves coming from planes of  $de/dz$  on the left, producing, say,  $H_1$ , and of  $H_2$  due to the negative waves from the planes of  $de/dz$  on the right side, making the complete solution

$$\left. \begin{aligned} H &= H_1 + H_2, \\ E &= \mu v (H_1 - H_2); \end{aligned} \right\} \quad . \quad . \quad . \quad . \quad . \quad (20)$$

where

$$H_1 = \frac{1}{2\mu v} \int_{-\infty}^z \frac{d}{dz'} f\left(t - \frac{z-z'}{v}, z'\right) dz', \quad . \quad . \quad (21)$$

$$H_2 = \frac{1}{2\mu v} \int_z^{\infty} \frac{d}{dz'} f\left(t + \frac{z+z'}{v}, z'\right) dz' \quad . \quad . \quad (22)$$

This is the most rational form of solution, and includes the case of  $e=f(t)$  only. The former may be derived from it by effecting the integrations in (21) and (22); remembering in doing so that the differential coefficient under the sign of integration is not the complete one with respect to  $z'$ , as it occurs twice, but only to the second  $z'$ , and further assuming that  $e=0$  at infinity.

4. *Waves in a Conducting Dielectric. How to remove the Distortion due to the Conductivity.*—Let us introduce a new physical property into the conducting medium, namely that it cannot support magnetic force without dissipation of energy at a rate proportional to the square of the force, a property which is the magnetic analogue of electric conductivity. We make the equations (2) and (3) become, if  $p=d/dt$ ,

$$\text{curl } \mathbf{H} = (4\pi k + cp) \mathbf{E}, \quad . \quad . \quad . \quad . \quad (23)$$

$$-\text{curl } \mathbf{E} = (4\pi g + \mu p) \mathbf{H}; \quad . \quad . \quad . \quad . \quad (24)$$

if there be no impressed force at the spot, where  $g$  is the new coefficient of magnetic conductivity, analogous to  $k$ .

Let

$$\left. \begin{aligned} 4\pi k/2c &= q_1, & 4\pi g/2\mu &= q_2, \\ q_1 + q_2 &= q, & q_1 - q_2 &= s, \\ \mathbf{E} &= \epsilon^{-qt} \mathbf{E}_1, & \mathbf{H} &= \epsilon^{-qt} \mathbf{H}_1. \end{aligned} \right\} \quad . \quad . \quad . \quad (25)$$

Substitution in (23), (24) lead to

$$\text{curl } \mathbf{H}_1 = c(s+p) \mathbf{E}_1, \quad . \quad . \quad . \quad . \quad (26)$$

$$-\text{curl } \mathbf{E}_1 = \mu(-s+p) \mathbf{H}_1. \quad . \quad . \quad . \quad . \quad (27)$$

If  $s=0$ , these are the equations of electric and magnetic force in a nonconducting dielectric. If therefore the new  $g$  be of such magnitude as to make  $s=0$ , we cause disturbances to be propagated in the conducting dielectric in identically the same manner as if it were nonconducting, but with a uniform attenuation at a rate indicated by the time-factor  $\epsilon^{-qt}$ .

5. *Undistorted Plane Waves in a Conducting Dielectric.*—Taking  $z$  perpendicular to the plane of the waves, we now have, as special forms of (23), (24),

$$-\frac{dH}{dz} = (4\pi k + cp)E, \quad . \quad . \quad . \quad (28)$$

$$-\frac{dE}{dz} = (4\pi g + \mu p)H, \quad . \quad . \quad . \quad (29)$$

$E$  being the tensor of  $\mathbf{E}$ , parallel to  $x$ , and  $H$  the tensor of  $\mathbf{H}$ , parallel to  $y$ , and both being functions of  $z$  and  $t$ .

Given  $E=E_0$  and  $H=H_0$  at time  $t=0$ , functions of  $z$  only, decompose them thus,

$$2f_1 = E_0 + \mu v H_0, \quad . \quad . \quad . \quad (30)$$

$$2f_2 = E_0 - \mu v H_0. \quad . \quad . \quad . \quad (31)$$

Here  $f_1$  makes the positive and  $f_2$  the negative wave, and at time  $t$  the solutions are, due to the initial state, when  $s=0$ ,

$$E = \epsilon^{-qt} \{f_1(z-vt) + f_2(z+vt)\}, \quad . \quad . \quad (32)$$

$$\mu v H = \epsilon^{-qt} \{f_1(z-vt) - f_2(z+vt)\}. \quad . \quad . \quad (33)$$

The only difference from plane waves in a nonconducting dielectric is in the uniform attenuation that goes on, due to the dissipation of energy, which is so balanced on the electric and magnetic sides as to annihilate the distortion the waves would undergo were  $s$  finite, whether positive or negative.

6. *Practical Application. Imitation of this Effect.*—When I introduced\* the new property of matter symbolized by the coefficient  $g$ , it was merely to complete the analogy between the electric and magnetic sides of electromagnetism. The property is non-existent, so far as I know. But I have more recently found how to precisely imitate its effect in another electromagnetic problem, also relating to plane waves, making use of electric conductivity to effect the functions of both  $k$  and  $g$  in §§ 4 and 5. In the case of § 5, first remove both conductivities, so that we have plane waves unattenuated and undistorted. Next put a pair of parallel wires of no resistance in the dielectric, parallel to  $z$ , and let the lines of electric force

\* See second footnote, p. 130.

terminate upon them, whilst those of magnetic force go round the wires. We shall still have these plane electromagnetic waves with curved lines of force propagated undistorted and unattenuated, at the same speed  $v$ . If  $V$  be the line-integral of  $\mathbf{E}$  across the dielectric from one wire to the other, and  $4\pi C$  be the line-integral of  $\mathbf{H}$  round either wire, we shall have

$$-\frac{dV}{dz} = LpC, \quad . \quad . \quad . \quad . \quad . \quad (34)$$

$$-\frac{dC}{dz} = SpV, \quad . \quad . \quad . \quad . \quad . \quad (35)$$

(34) taking the place of (29), and (35) of (28), with  $k$  and  $g$  both zero. Here  $L$  and  $S$  are the inductance and permittance of unit length of the circuit of the parallel wires, and  $v = (LS)^{-\frac{1}{2}}$ .

Next let the wires have constant resistance  $R$  per unit length to current in them, and let the medium between them be conducting (to a very low degree), making  $K$  the conductance per unit length across from one wire to the other. We then turn the last equations into

$$-\frac{dV}{dz} = (R + Lp)C, \quad . \quad . \quad . \quad . \quad . \quad (36)$$

$$-\frac{dC}{dz} = (K + Sp)V, \quad . \quad . \quad . \quad . \quad . \quad (37)$$

and have a complete imitation of the previous unreal problem. The two dissipations of energy are now due to  $R$  in the wires, and to  $K$  in the dielectric, it being that in the wires which takes the place of the unreal magnetic dissipation. The relation  $R/L = K/S$ , which does not require excessive leakage when the wires are of copper of low resistance, removes the distortion otherwise suffered by the waves. I have, however, found that when the alternations of current are very rapid, as in telephony, there is very little distortion produced by copper wires, even without the leakage required to wholly remove it, owing to  $R/Ln$  becoming small,  $n/2\pi$  being the frequency; an effect which is greatly assisted by increasing the inductance (see Note A, p. 151). Of course there is little resemblance between this problem and that of the long and slowly-worked submarine cable, whether looked at from the physical side or merely from the numerical point of view, the results being then of different orders of magnitude. A remarkable misconception on this point seems to be somewhat generally held. It seems to be imagined that self-induction



is harmful\* to long-distance telephony. The precise contrary is the case. It is the very life and soul of it, as is proved both by practical experience in America and the Continent on very long copper circuits, and by examining the theory of the matter. I have proved this in considerable detail†; but they will not believe it. So far does the misconception extend that it has perhaps contributed to leading Mr. W. H. Preece to conclude that the coefficient of self-induction in copper circuits is negligible (several hundred times smaller than it can possibly be), on the basis of his recent remarkable experimental researches.

The following formula, derived from my general formulæ‡, will show the rôle played by self-induction. Let  $R$  and  $L$  be the resistance and inductance per unit length of a perfectly insulated circuit of length  $l$ , short-circuited at both ends. Let a rapidly sinusoidal impressed force act at one end of amplitude  $e_0$ , and let  $C_0$  be the amplitude of the current at the distant end. Then, if the circuit be very long,

$$C_0 = \frac{2e_0}{Lv} e^{-Rl/2Lv}, \quad . \quad . \quad . \quad . \quad . \quad (38)$$

where  $v$  is the speed  $(LS)^{-\frac{1}{2}} = (\mu c)^{-\frac{1}{2}}$ , provided  $R/Ln$  be small, say  $\frac{1}{4}$ . It may † be considerably greater, and yet allow (38) to be nearly true. We can include nearly the whole range of telephonic frequencies by using suspended copper wires of low resistance§.

It is resistance that is so harmful, not self-induction; as, in combination with the electrostatic permittance, it causes immense distortion of waves, unless counteracted by increasing the inductance, which is not often practicable (see Note B, p. 152).

7. *Distorted Plane Waves in a Conducting Dielectric.*—Owing to the fact that, as above shown, we can fully utilize solutions involving the unreal  $g$ , by changing the meaning of the symbols, whilst still keeping to plane electromagnetic waves, we may preserve  $g$  in our equations (28) and (29), remembering that  $H$  has to become  $C$ ,  $E$  become  $V$ ,  $4\pi k$

\* W. H. Preece, F.R.S., "On the Coefficient of Self-Induction of Copper Wires," B. A. Meeting, 1887.

† "El. Mag. Ind. and its Propagation," *Electrician*, Arts. xl. to l. (1887).

‡ See the sinusoidal solutions in Part II. and Part V. of "On the Self-Induction of Wires," *Phil. Mag.* Sept. 1886 and Jan. 1887.

§ The explanation of the  $\frac{1}{2}Lv$  dividing  $e_0$  in (38), instead of the  $Lv$  we might expect from the  $\mu v$  resistance-operator of a tube of unit section infinitely long one way only, is that, on arrival at the distant end of the line, the current is immediately doubled in amplitude by the reflected wave. The second and following reflected waves are negligible, on account of the length of the line.

become  $K$ ,  $c$  become  $S$ ,  $4\pi g$  become  $R$ , and  $\mu$  become  $L$ , when making the application to the possible problem; whilst, when dealing with a real conducting dielectric,  $g$  has to be zero.

Required the solutions of (28) and (29) due to any initial states  $E_0$  and  $H_0$ , when  $s$  is not zero. Using the notation and transformations of (25), (or direct from (26), (27)), we produce

$$-\frac{dH_1}{dz} = c(s+p)E_1, \quad . \quad . \quad . \quad (39)$$

$$-\frac{dE_1}{dz} = \mu(-s+p)H_1; \quad . \quad . \quad . \quad (40)$$

from which

$$v^2 \frac{d^2 H_1}{dz^2} = (p^2 - s^2)H_1, \quad . \quad . \quad . \quad (41)$$

with the same equation for  $E_1$ .

The complete solution may be thus described. Let, at time  $t=0$ , there be  $H=H_0$  through the small distance  $a$  at the origin. This immediately splits into two plane waves of half the amplitude, which travel to right and left respectively at speed  $v$ , attenuating as they progress, so that at time  $t$  later, when they are at distances  $\pm vt$  from the origin, their amplitudes equal

$$\frac{1}{2} H_0 \epsilon^{-qt}, \quad . \quad . \quad . \quad (42)$$

with corresponding  $E$ 's, viz.

$$\frac{1}{2} \mu v H_0 \epsilon^{-qt} \quad \text{and} \quad -\frac{1}{2} \mu v H_0 \epsilon^{-qt}, \quad . \quad . \quad . \quad (43)$$

on the right and left sides respectively. These extend through the distance  $a$ . Between them is a diffused disturbance, given by

$$H = \epsilon^{-qt} \frac{H_0 a}{2v} \left( s + \frac{d}{dt} \right) J_0 \left\{ \frac{s}{v} (z^2 - v^2 t^2)^{\frac{1}{2}} \right\}, \quad . \quad . \quad . \quad (44)$$

$$E = \epsilon^{-qt} \frac{H_0 a}{2cv} \left( -\frac{d}{dz} \right) J_0 \left\{ \frac{s}{v} (z^2 - v^2 t^2)^{\frac{1}{2}} \right\}, \quad . \quad . \quad . \quad (45)$$

in which  $v^2 t^2 > z^2$ .

In a similar manner, suppose initially  $E=E_0$  through distance  $a$  at the origin. Then, at time  $t$  later, we have two plane strata of depth  $a$  at distance  $vt$  to right and left respectively, in which

$$E = \frac{1}{2} E_0 \epsilon^{-qt} = \pm \mu v H, \quad . \quad . \quad . \quad (46)$$

the  $+$  sign to be used in the right-hand stratum, the  $-$  in the left. And, between them, the diffused disturbance

given by

$$E = \epsilon^{-qt} \frac{E_0 a}{2v} \left( -s + \frac{d}{dt} \right) J_0 \left\{ \frac{s}{v} (z^2 - v^2 t^2)^{\frac{1}{2}} \right\}, \quad . \quad . \quad (47)$$

$$H = \epsilon^{-qt} \frac{E_0 a}{2\mu v} \left( -\frac{d}{dz} \right) J_0 \left\{ \frac{s}{v} (z^2 - v^2 t^2)^{\frac{1}{2}} \right\}. \quad . \quad . \quad . \quad (48)$$

Knowing thus the effects due to initial elements of  $E_0$  and  $H_0$ , we have only to integrate with respect to  $z$  to find the solutions due to any arbitrary initial distributions. I forbear from giving a detailed demonstration, leaving the satisfaction of the proper conditions to be the proof of (42) to (48); since, although they were very laboriously worked out by myself, yet, as mathematical solutions, are more likely to have been given before in some other physical problem than to be new.

Another way of viewing the matter is to start with  $s=0$ , and then examine the effect of introducing  $s$ , either  $+$  or  $-$ . Let an isolated plane disturbance of small depth be travelling along in the positive direction undistorted at speed  $v$ . We have  $E = \mu v H$  in it. Now suddenly increase  $k$ , making  $s$  positive. The disturbance still keeps moving on at the same speed, but is attenuated with greater rapidity. At the same time it leaves a tail behind it, the tip of which travels out the other way at speed  $v$ , so that at time  $t$ , after commencement of the tailing, the whole disturbance extends through the distance  $2vt$ . In this tail  $H$  is of the same sign as in the head, and its integral amount is such that it exactly accounts for the extra-attenuation suffered by  $H$  in the head. On the other hand,  $E$  in the tail is of the opposite sign to  $E$  in the head; so that the integral amount of  $E$  in head and tail decreases faster. As a special case, let, in the first place, there be no conductivity,  $k=0$  and  $g=0$ . Then, keeping  $g$  still zero, the effect of introducing  $k$  is to cause the above-described effect, except that as there was no attenuation at first, the attenuation later is entirely due to  $k$ , whilst the line-integral of  $H$  along the tail, or

$$\int H dz,$$

including  $H$  in the head, remains constant. This is the persistence of momentum.

If, on the other hand, we introduce  $g$ , the statements made regarding  $H$  are now true as regards  $E$ , and conversely. The tail is of a different nature,  $E$  being of same sign in the tail as in the head, and  $H$  of the opposite sign. Hence, of course, when we have both  $k$  and  $g$  of the right amounts, there is no

tailing. This subject is, however, far better studied in the telegraphic application, owing to the physical reality then existent, than in the present problem, and also then by elementary methods\*.

8. Owing to the presence of  $d/dz$  in (45) and (47) we are enabled to give some integral solutions in a finite form. Thus, let  $H=H_0$  constant and  $E=0$  initially on the whole of the negative side of the origin, with no  $E$  or  $H$  on the positive side. The  $E$  at time  $t$  later is got by integrating (45), giving

$$E = \frac{H_0}{2cv} J_0 \left\{ \frac{s}{v} (z^2 - v^2 t^2)^{\frac{1}{2}} \right\} e^{-qt}, \quad . \quad . \quad . \quad (49)$$

which holds between the limits  $z = \pm vt$ , there being no disturbance beyond, except the  $H_0$  on the left side. When  $g=0$  and  $z/vt$  is small, it reduces to

$$E = \frac{H_0}{4\pi} \left( \frac{\mu}{kt} \right)^{\frac{1}{2}} e^{-\pi k \mu z^2 / t}. \quad . \quad . \quad . \quad (50)$$

This is the pure diffusion-solution, suitable for good conductors.

If initially  $E=E_0$ , constant, on the left side of the origin, and zero on the right side, then at time  $t$  the  $H$  due to it is, by (48),

$$H = \frac{E_0}{2\mu v} J_0 \left\{ \frac{s}{v} (z^2 - v^2 t^2)^{\frac{1}{2}} \right\} e^{-qt}. \quad . \quad . \quad . \quad (51)$$

The result of taking  $c=0$ ,  $g=0$ , in this formula is zero, as we may see by observing that  $c$  in (49) becomes  $\mu$  in (51). It is of course obvious that, as the given initial electric field has no energy if  $c=0$ , it can produce no effect later.

The  $H$  solution corresponding to (49) cannot be finitely expressed. It is

$$H = \frac{1}{2} H_0 e^{-qt} \left[ 1 + \int_z^{vt} \frac{dx}{v} \left( s + \frac{d}{dt} \right) J_0 \left\{ \frac{s}{v} (x^2 - v^2 t^2)^{\frac{1}{2}} \right\} dx \right],$$

which, integrated, gives

$$H = \frac{1}{2} H_0 e^{-qt} \left[ \epsilon^{st} - \frac{sz}{v} (J_0 - iJ_1) + \frac{1}{3} \left( \frac{sz}{v} \right)^3 \frac{1}{st} (-iJ_1 - J_2) \right. \\ \left. - \frac{1}{5} \cdot 3 \left( \frac{sz}{v} \right)^5 \frac{1}{s^2 t^2} (-J_2 + iJ_3) + \frac{1}{7} \cdot 3 \cdot 5 \left( \frac{sz}{v} \right)^7 \frac{1}{s^3 t^3} (iJ_3 + J_4) + \dots \right], \quad (52)$$

\* "Electro-magnetic Induction and its Propagation," Electrician, Arts. xlii. to l. (1887).



where all the  $J$ 's operate on  $st\sqrt{-1}$ ; thus, *e. g.* (Bessel's),

$$J_3 = J_3(st\sqrt{-1}).$$

But a much better form than (52), suitable for calculating the shape of the wave speedily, especially at its start, may be got by arranging in powers of  $z-vt$ , thus

$$H = \frac{1}{2}H_0\epsilon^{-qt} \left\{ 1 + stf_1\left(1 - \frac{z}{vt}\right) + \frac{s^2t^2}{2}f_2\left(1 - \frac{z}{vt}\right)^2 + \frac{s^3t^3}{6}f_3\left(1 - \frac{z}{vt}\right)^3 + \dots \right\}, \quad (53)$$

true when  $z < vt$ , where  $f_1, f_2$ , &c. are functions of  $t$  only, of which the first five are given by

$$f_1 = 1 + \frac{st}{2},$$

$$f_2 = \frac{st}{2}\left(1 + \frac{st}{4}\right),$$

$$f_3 = -\frac{1}{2}\left(1 + \frac{st}{4}\right) + \frac{s^2t^2}{2 \cdot 4}\left(1 + \frac{st}{6}\right),$$

$$f_4 = -\frac{3}{4}\frac{st}{2}\left(1 + \frac{st}{6}\right) + \frac{s^3t^3}{2 \cdot 4 \cdot 6}\left(1 + \frac{st}{8}\right),$$

$$f_5 = \frac{3}{8}\left(1 + \frac{st}{6}\right) - \frac{s^2t^2}{2 \cdot 4}\left(1 + \frac{st}{8}\right) + \frac{s^4t^4}{2 \cdot 4 \cdot 6 \cdot 8}\left(1 + \frac{st}{10}\right).$$

At the origin,  $H$  is given by

$$H = \frac{1}{2}H_0\epsilon^{-2q_2t}, \quad (54)$$

and is therefore permanently  $\frac{1}{2}H_0$  when  $g=0$ . At the front of the wave, where  $z=vt$ ,

$$H = \frac{1}{2}H_0\epsilon^{-qt}. \quad (55)$$

Now, to represent the  $E$  solution corresponding to (51), we have only to turn  $H_0$  to  $E_0$  in (53), and change the sign of  $s$  throughout, *i. e.* explicit, and in the  $f$ 's. Similarly in (52). Thus, at the origin,

$$E = \frac{1}{2}E_0\epsilon^{-2q_1t}, \quad (56)$$

and at the front of the wave

$$E = \frac{1}{2}E_0\epsilon^{-qt}. \quad (57)$$

9. Again, let  $H = \frac{1}{2}H_0$  on the left side, and  $H = -\frac{1}{2}H_0$  on the right side of the origin, initially. The  $E$  that results from each of them is the same, and is half that of (49); so that (49) still expresses the  $E$  solution. This case corresponds to an initial electric current of surface-density  $H_0/4\pi$  on the

$z=0$  plane, with the full magnetic field to correspond, and from it immediately follows the  $\mathbf{E}$  solution due to any initial distribution of electric current in plane layers.

Owing to  $\mathbf{H}$  being permanently  $\frac{1}{2}\mathbf{H}_0$  at the origin in the case (49), (54), when  $g=0$ , we may state the problem thus: An infinite conducting dielectric with a plane boundary is initially free from magnetic induction, and its boundary suddenly receives the magnetic force  $\mathbf{H}_0=\text{constant}$ . At time  $t$  later (49) and (52) or (53) give the state of the conductor at distance  $z<vt$  from the boundary. In a good conductor the attenuation at the front of the wave is so enormous that the diffusion solution (50) applies practically. It is only in bad conductors that the more complete form is required.

10. *Effect of Impressed Force.*—We can show that the initial effect of impressed force is the same as if the dielectric were nonconducting. In equations (23), (24), let  $p=ni$ , where  $n2\pi$ =frequency of alternations, supposing  $\mathbf{e}$  to alternate rapidly. By increasing  $n$  we can make the second terms on the right sides be as great multiples of the first terms as we please, so that in the limit we have results independent of  $k$  and  $g$ , in this respect, that as the frequency is raised infinitely, the true solutions tend to be infinitely nearly represented by simplified forms, in which  $k$  and  $g$  play the part of small quantities. An inspection of the sinusoidal solution for plane waves shows that  $\mathbf{E}$  and  $\mathbf{H}$  get into the same phase, and that  $k$  and  $g$  merely present themselves in the exponents of factors representing attenuation of amplitude as the waves pass away from the seat of vorticity of impressed force.

Consequently, in the plane problem, the initial effect of an abrupt discontinuity in  $e$ , say  $e=\text{constant}$  on the left, and zero on the right side of the plane through the origin, is to produce

$$\mathbf{H} = -e/2\mu v \quad . \quad . \quad . \quad . \quad . \quad . \quad (58)$$

all over the plane of vorticity; and

$$\mathbf{E} = \mp \frac{1}{2} e \quad . \quad . \quad . \quad . \quad . \quad . \quad (59)$$

on its left and right sides respectively. We may regard the plane as continuously emitting these disturbances to right and left at speed  $v$  so long as the impressed force is in operation, but their subsequent history can only be fully represented by the tail formulæ already given.

Irrespective of the finite curvature of a surface, any element thereof may be regarded as plane. Therefore every element of a sheet of vortex lines of impressed force acts in the way just described as being true of the elements of an infinite plane sheet. But it is only in comparatively simple

cases, of which I shall give examples later, that the subsequent course of events does not so greatly complicate matters as to render it impossible to go into details after the first moment. On first starting the sheet, it becomes a sheet of magnetic induction, whose lines coincide with the vortex lines of impressed force. If  $f$  be the measure of the vorticity per unit area,  $f/2\mu v$  is the intensity of the magnetic force. In the imaginary good conductor of *no* permittivity, this is zero, owing to  $v$  being then assumed to be infinite.

Notice that whilst the vorticity of  $\mathbf{e}$  produces magnetic induction, that of  $\mathbf{h}$  produces electric displacement, and whilst in the former case  $\mathbf{E}$  is made discontinuous at a plane of finite vorticity, in the latter case it is  $\mathbf{H}$  that is initially discontinuous.

11. *True Nature of Diffusion in Conductors.*—The process of diffusion of magnetic induction in conductors appears to be fundamentally one of repeated internal reflexions with partial transmission. Thus, let a plane wave  $\mathbf{E}_1 = \mu v \mathbf{H}_1$  moving in a nonconducting dielectric strike flush an exceedingly thin sheet of metal. Let  $\mathbf{E}_2 = \mu v \mathbf{H}_2$  be the transmitted wave in the dielectric on the other side, and  $\mathbf{E}_3 = -\mu v \mathbf{H}_3$  be the reflected wave. At the sheet we have

$$\mathbf{E}_1 + \mathbf{E}_3 = \mathbf{E}_2, \quad \dots \dots \dots (60)$$

$$\mathbf{H}_1 + \mathbf{H}_3 = \mathbf{H}_2 + 4\pi k_1 z \mathbf{E}_2, \quad \dots \dots \dots (61)$$

if  $k_1$  be the conductivity of the sheet of thickness  $z$ . Therefore

$$\frac{\mathbf{E}_2}{\mathbf{E}_1} = \frac{\mathbf{H}_2}{\mathbf{H}_1} = \frac{\mathbf{E}_1 + \mathbf{E}_3}{\mathbf{E}_1} = \frac{1}{1 + 2\pi\mu k_1 z v}. \quad \dots \dots (62)$$

$\mathbf{H}$  is reflected positively and  $\mathbf{E}$  negatively. A perfect conducting barrier is a perfect reflector, it doubles the magnetic force and destroys the electric force on the side containing the incident wave, and transmits nothing.

Take  $k_1 = (1600)$  for copper, and  $\mu v = 3 \times 10^{10}$  centim.

Then we see that to attenuate the incident wave  $\mathbf{H}_1$  to  $\frac{1}{2}\mathbf{H}_1$  by transmission through the plate, requires

$$z = (2\pi\mu k_1 v)^{-1} = \frac{8}{3\pi} \times 10^{-8} \text{ centim.}, \quad \dots (63)$$

which is a very small fraction of the wave-length of visible light. The  $\mathbf{H}$  disturbance is made  $\frac{3}{2}\mathbf{H}_1$ , the  $\mathbf{E}$  reduced to  $\frac{1}{2}\mathbf{E}_1$  on the transmission side. There is, however, persistence of  $\mathbf{H}$ , although there is dissipation of  $\mathbf{E}$ . To produce dissipation of  $\mathbf{H}$  with persistence of  $\mathbf{E}$  requires the plate to be a magnetic, not an electric conductor.

Now, imagine an immense number of such plates to be

packed closely together, with dielectric between them, forming a composite dielectric conductor, and let the outermost sheet be struck flush by a plane wave as above. The first sheet transmits  $\frac{1}{2}H_1$ , the second  $\frac{1}{4}H_1$ , the third  $\frac{1}{8}H_1$ , and so on. This refers to the front of the wave, going into the composite conductor at speed  $v$ . It is only necessary to go a very short distance to attenuate the front of the wave to nothing; the immense speed of propagation does not result in producing any sensible immediate effect at a distance, which comes on quite slowly as the complex result of all the internal reflexions and transmissions between and at the sheets. Observe that there is an initial accumulation of  $H$ , so to speak, at the boundary of the conductor, due to the reflexion. [Example: the current-density may be greater at the outermost layer\* of a round wire when the current is started in it than the final value, and the total current in the wire increases faster than if it were constrained to be uniformly distributed.]

Thus a good conductor may have very considerable permittivity, much greater than that of air, and yet show no signs of it, on account of the extraordinary attenuation produced by the conductivity. Now this is rather important from the theoretical point of view. It is commonly assumed that good conductors, *e.g.* metals, are not dielectrics at all. This makes the speed of propagation of disturbances through them infinitely great. Such a hypothesis, however, should have no place in a rational theory, professing to represent transmission in time by stresses in a medium occupying the space between molecules of gross matter. But by admitting that not only bad conductors, but all conductors, are also dielectrics, we do away with the absurdity of infinitely rapid action through infinite distances in no time at all, and make the method of propagation, although it practically differs so greatly from that in a nonconducting dielectric, be yet fundamentally the same, with its characteristic features masked by repeated internal reflexions with loss of energy. We need not take any account of the electric displacement in actual reckonings of the magnitude of the effects which can be observed in the case of good conductors, but it is surely a mistake to overlook it when it is the nature of the actions involved that is in question. (See Note C, p. 153).

Why conductors act as reflectors is quite another question, which can only be answered speculatively. If molecules are perfect conductors, they are perfect reflectors, and if they were packed quite closely, we should nearly have a perfect conductor in mass, impenetrable by magnetic induction; and

\* "On the S. I. of Wires," Part I. *Phil. Mag.* August 1886.



we know that cooling a metal and packing the molecules closer does increase its conductivity. But as they do not form a compact mass in any substance, they must always allow a partial transmission of electromagnetic waves in the intervening dielectric medium, and this would lead to the diffusion method of propagation. We do not, however, account in this way for the dissipation of energy, which requires some special hypothesis.

The diffusion of heat, too, which is, in Fourier's theory, done by instantaneous action to infinite distances, cannot be physically true, however insignificant may be the numerical departures from the truth. What can it be but a process of radiation, profoundly modified by the molecules of the body, but still only transmissible at a finite speed? The very remarkable fact that the more easily penetrable a body is to magnetic induction the less easily it conducts heat, in general, is at present a great difficulty in the way, though it may perhaps turn out to be an illustration of electromagnetic principles eventually.

12. *Infinite Series of Reflected Waves. Remarkable Identities. Realized Example.*—When, in a plane-wave problem, we confine ourselves to the region between two parallel planes, we can express our solutions in Fourier series, constructed so as to harmonize with the boundary conditions which represent the effect of the whole of the ignored regions beyond the boundaries in modifying the phenomena occurring within the limited region. Now the effect of the boundaries is usually to produce reflected waves. Hence a solution in Fourier series must usually be decomposable into an infinite series of separate solutions, coming into existence one after the other in time if the speed  $v$  be finite, or all in operation at once from the first moment if the speed be made infinite (as in pure diffusion). If the boundary conditions be of a simple nature, this decomposition can sometimes be easily explicitly represented, indicating remarkable identities, of which the following investigation leads to one. We may either take the case of plane waves in a conducting dielectric bounded by infinitely conductive planes, making  $E=0$  the boundary condition; or, similarly, by infinitely inductive planes producing  $H=0$  at them. But the most practical way, and the most easily followed, is to put a pair of parallel wires in the dielectric, and produce a real problem relating to a telegraph-circuit.

Let A and B be its terminations at  $z=0$  and  $z=l$  respectively. Let them be short-circuited, producing the terminal conditions  $V=0$  at A and B in the absence of impressed force at either place. Now, the circuit being free from charge

and current initially, insert a steady impressed force  $e_0$  at A. Required the effect, both in Fourier series and in detail, showing the whole history of the phenomena that result.

Equations (36) and (37) are the fundamental connexions of  $V$  and  $C$  at any distance  $z$  from A. Let  $R$ ,  $L$ ,  $K$ ,  $S$  be the resistance, inductance, leakage-conductance, and permittance per unit length of circuit, and

$$s_1 = R/2L, \quad s_2 = K/2S, \quad q = s_1 + s_2, \quad s_0 = s_1 - s_2. \quad (64)$$

$$\lambda = (m^2 v^2 - s_0^2)^{\frac{1}{2}}. \quad . \quad . \quad . \quad (65)$$

It may be easily shown, by the use of the resistance operator, or by testing satisfaction of conditions, that the required solutions are

$$V = V_0 - \frac{2e_0}{l} \sum \frac{m \sin mz}{RK + m} \epsilon^{-qt} \left[ \cos \lambda t + \frac{q}{\lambda} \sin \lambda t \right], \quad . \quad . \quad . \quad (66)$$

$$C = C_0 - \frac{e_0}{Rl} \epsilon^{-2s_1 t} - \frac{2e_0}{Rl} \sum \frac{\cos mz}{m^2 + RK} \epsilon^{-qt} \left[ RK \left( \cos - \frac{s_0}{\lambda} \sin \right) \lambda t \right. \\ \left. - 2s_1 m^2 \frac{\sin \lambda t}{\lambda} \right], \quad . \quad . \quad (67)$$

where  $m = j\pi/l$ , and  $j$  includes all integers from 1 to  $\infty$ ; whilst  $V_0$  and  $C_0$  represent the final steady  $V$  and  $C$ , which are

$$V_0 = e_0 \left( \cos m_0 z - \frac{\sin m_0 z}{\tan m_0 l} \right), \quad . \quad . \quad . \quad (68)$$

$$C_0 = \frac{m_0 e_0}{R} \left( \sin m_0 z + \frac{\cos m_0 z}{\tan m_0 l} \right), \quad . \quad . \quad . \quad (69)$$

where  $m_0^2 = -RK$ .

Now if the circuit were infinitely long both ways and were charged initially to potential-difference  $2e_0$  on the whole of the negative side of A, with no charge on the positive side, and no current anywhere, the resulting current at time  $t$  later at distance  $z$  from A would be

$$C_1 = \frac{e_0}{Lv} \epsilon^{-qt} J_0 \left\{ \frac{s_0}{v} (z^2 - v^2 t^2)^{\frac{1}{2}} \right\}, \quad . \quad . \quad (70)$$

by §§ 7 and 8; and if, further,  $K=0$ ,  $V$  at A would be permanently  $e_0$ , which is what it is in (66). Hence the  $C$  solution

(67) can be finitely decomposed into separate solutions of the form (70) in the case of perfect insulation, when (67) takes the form

$$C = \frac{e_0}{Rl}(1 - e^{-2qt}) + \frac{2e_0}{Ll} e^{-qt} \sum \cos mz \frac{2q}{\lambda} \sin \lambda t, \quad (71)$$

where  $q = s_1 = s_0$ , by the vanishing of  $s_2$  in (64).

Therefore (70) represents the real meaning of (71) from  $t=0$  to  $l/v$ , provided  $vt > z$ . But on arrival of the wave  $C_1$  at B,  $V$  becomes zero, and  $C$  doubled by the reflected wave that then commences to travel from B to A. This wave may be imagined to start when  $t=0$  from a point distant  $l$  beyond B, and be the precise negative of the first wave as regards  $V$  and the same as regards  $C$ . Thus

$$C_2 = \frac{e_0}{Lv} e^{-qt} J_0 \left\{ \frac{q}{v} [(2l - z)^2 - v^2 t^2]^{\frac{1}{2}} \right\}, \quad (72)$$

expresses the second wave, starting from B when  $t=l/v$ , and reaching A when  $t=2l/v$ . The sum of  $C_1$  and  $C_2$  now expresses (71), where the waves coexist, and  $C_1$  alone expresses (71) in the remainder of the circuit.

The reflected wave arising when this second wave reaches A may be imagined to start when  $t=0$  from a point distant  $2l$  from A on its negative side, and be a precise copy of the first wave. Thus

$$C_3 = \frac{e_0}{Lv} e^{-qt} J_0 \left\{ \frac{q}{v} [(2l + z)^2 - v^2 t^2]^{\frac{1}{2}} \right\}, \quad (73)$$

expresses the third wave; and now (71) means  $C_1 + C_2 + C_3$  in those parts of the circuit reached by  $C_3$  and  $C_1 + C_2$  in the remainder.

The fourth wave is, similarly,

$$C_4 = \frac{e_0}{Lv} e^{-qt} J_0 \left\{ \frac{q}{v} [(4l - z)^2 - v^2 t^2]^{\frac{1}{2}} \right\}, \quad (74)$$

starting from B when  $t=3l/v$ , and reaching A when  $t=4l/v$ . And so on, *ad inf.*\*

\* It is not to be expected that in a real telegraph-circuit the successive waves have abrupt fronts, as in the text. There are causes in operation to prevent this, and round off the abruptness. The equations connecting

If we take  $L=0$  in this problem, we make  $v=\infty$ , and bring the whole of the waves into operation immediately. (70) becomes

$$C_1 = e_0 \left( \frac{S}{\pi R l} \right)^{\frac{1}{2}} \epsilon^{-RSz^2/4t}; \quad . \quad . \quad . \quad (75)$$

and similarly for  $C_2$ ,  $C_3$ , &c. In this simplified form the identity is that obtained by Sir W. Thomson\* in connexion with his theory of the submarine cable; also discussed by A. Cayley\* and J. W. L. Glaisher†.

In order to similarly represent the history of the establishment of  $V_0$ , we require to use the series (53) or some equivalent. In other respects there is no difference.

Whilst it is impossible not to admire the capacity possessed by solutions in Fourier series to compactly sum up the effect of an infinite series of successive solutions, it is greatly to be regretted that the Fourier solutions themselves should be of such difficult interpretation. Perhaps there will be discovered some practical way of analyzing them into easily interpretable forms.

Some special cases of (66), (67) are worthy of notice. Thus  $V$  is established in the same way when  $R=0$  as when  $K=0$ , provided the value of  $K/S$  in the first case be the same as that of  $R/L$  in the second. Calling this value  $2q$ , we have in both cases

$$V = e_0 \left( 1 - \frac{z}{l} \right) - \frac{2e_0}{l} \epsilon^{-qt} \sum \frac{\sin mz}{m} \left( \cos \lambda t + \frac{q}{\lambda} \sin \lambda t \right). \quad (76)$$

But the current is established in quite different manners. When it is  $K$  that is zero, (71) is the solution; but if  $R$  vanish instead, then (67) gives

$$C = \frac{e_0 t}{Ll} + \frac{e_0 K l}{2} \left( 1 - \frac{z}{l} \right)^2 - \frac{2e_0 K}{l} \epsilon^{-qt} \sum \frac{\cos mz}{m^2} \left\{ \cos \lambda t - \left( \frac{m^2 t^2}{2q} - q \right) \frac{\sin \lambda t}{\lambda} \right\}. \quad (77)$$

$V$  and  $C$  express the first approximation to a complete theory. Thus the wires are assumed to be instantaneously penetrated by the magnetic induction as a wave passes over their surfaces, as if the conductors were infinitely thin sheets of the same resistance. It is only a very partial remedy to divide a wire into several thinner wires, unless we at the same time widely separate them. If kept quite close it would, with copper, be no remedy at all.

\* Math. and Physical Papers, vol. ii. Art. lxxii.; with Note by A. Cayley.

† Phil. Mag. June 1874.



C now mounts up infinitely. But the leakage-current, which is  $KV$ , becomes steady, as (76) shows.

In connexion with this subject I should remark that the non-distortional circuit produced by taking  $R/L=K/S$  is of immense assistance, as its properties can be investigated in full detail by elementary methods, and are most instructive in respect to the distortional circuits in question above\*.

13. *Modifications made by Terminal Apparatus.* Certain cases easily brought to full realization.—Suppose that the terminal conditions in the preceding are  $V=-Z_0C$  and  $V=Z_1C$ ,  $Z_0$  and  $Z_1$  being the “resistance operators” of terminal apparatus at A and B respectively. In a certain class of cases the determinantal equation so simplifies as to render full realization possible in an elementary manner. Thus, the resistance-operator of the circuit, reckoned at A, is †

$$\phi = Z_0 + \frac{(R + Lp)l(\tan ml)/ml + Z_1}{1 + (K + Sp)lZ_1(\tan ml)/ml}, \quad (78)$$

where

$$m^2 = -(R + Lp)(K + Sp). \quad (79)$$

That is,  $e = \phi C$  is the linear differential equation of the current at A. Now, to illustrate the reductions obviously possible, let  $Z_0 = 0$ , and

$$Z_1 = n_1 l(R + Lp). \quad (80)$$

This makes the apparatus at B a coil whose time-constant is  $L/R$ , and reduces  $\phi$  to

$$\phi = (R + Lp)l \left( \frac{\tan ml}{ml} + n_1 \right) \left\{ 1 - m^2 n_1 l^2 \frac{\tan ml}{ml} \right\}^{-1}, \quad (81)$$

so that the roots of  $\phi = 0$  are given by

$$R + Lp = 0. \quad (82)$$

$$\tan ml + mln_1 = 0; \quad (83)$$

i. e. a solitary root  $p = -R/L$  and the roots of (83), which is an elementary well-known form of determinantal equation.

The complete solution due to the insertion of the steady impressed force  $e_0$  at A will be given by‡

\* “Electromagnetic Induction and its Propagation,” Arts. xl. to l.

† “On the Self-Induction of Wires,” Part IV.

‡ *Id.* Parts III. and IV. Phil. Mag. Oct. and Nov. 1886; or “On Resistance and Conductance Operators,” Phil. Mag. Dec. 1887, § 17, p. 500.

$$V = V_0 + \Sigma e_0 u \epsilon^{pt} \div \left( p \frac{d\phi}{dp} \right), \quad . \quad . \quad . \quad (84)$$

$$C = C_0 + \Sigma e_0 w \epsilon^{pt} \div \left( p \frac{d\phi}{dp} \right), \quad . \quad . \quad . \quad (85)$$

where the summations range over all the  $p$  roots of  $\phi=0$ , subject to (79); whilst  $u$  and  $w$  are the  $V$  and  $C$  functions in a normal system expressed by

$$w = \cos mz, \quad u = m \sin mz \div (K + Sp); \quad . \quad . \quad (86)$$

and  $V_0, C_0$  are the final steady  $V$  and  $C$ . In the case of the solitary root (82) we shall find

$$-p \frac{d\phi}{dp} = Rl(1 + n_1), \quad . \quad . \quad . \quad . \quad (87)$$

but for all the rest

$$-p \frac{d\phi}{dp} = \frac{l}{2(K + Sp)} \frac{dm^2}{dp} (1 + n_1 \cos^2 ml). \quad . \quad (88)$$

Realizing (84), (85) by pairing terms belonging to the two  $p$ 's associated with one  $m^2$  through (79), we shall find that (66), (67) express the solutions, provided we make these simple changes:—Divide the general term in both the summations by

$$1 + n_1 \cos^2 ml,$$

and the term following  $C_0$  outside the summation in (67) by  $(1 + n_1)$ . Of course the  $m$ 's have now different values, as per (83), and  $V_0, C_0$  are different.

14. There are several other cases in which similar reductions are possible. Thus, we may have

$$Z_0 = n_0(R + Lp) + n_0'(K + Sp)^{-1},$$

$$Z_1 = n_1(R + Lp) + n_1'(K + Sp)^{-1},$$

simultaneously,  $n_0, n_0', n_1, n_1'$  being any lengths. That is, apparatus at either end consisting of a coil and a condenser in sequence, the time-constant of the coil being  $L/R$  and that of the condenser  $S/K$ . Or, the condenser may be in parallel with the coil. In general we have, as an alternative form of  $\phi=0$ , equation (78),

$$\frac{\tan ml}{ml} = - \frac{(Z_0 + Z_1) \{ (R + Lp)l \}^{-1}}{1 - m^2 l^2 Z_0 Z_1 \{ (R + Lp)l \}^{-2}}; \quad . \quad . \quad (89)$$

from which we see that when

$$\frac{Z_0}{(R + Lp)l} \quad \text{and} \quad \frac{Z_1}{(R + Lp)l}$$

are functions of  $ml$ , equation (89) finds the value of  $m^2$  immediately, *i. e.* not indirectly as functions of  $p$ . In all such cases, therefore, we may advantageously have the general solutions (80), (81) put into the realized form. They are

$$V = V_0 - \frac{2e_0}{l} \sum \frac{(\sin mz + \tan \theta \cos mz)m\epsilon^{-qt}(\cos + q\lambda^{-1} \sin)\lambda t}{\sec^2 \theta (m^2 + RK) \left(1 - \cos^2 ml \frac{d}{d(ml)} \tan ml\right)}, \quad (90)$$

$$C = C_0 - \frac{2e_0}{l} \sum \frac{(\cos mz - \tan \theta \sin mz)\epsilon^{-qt}K \{\cos - (2s_2\lambda)^{-1}(\lambda^2 + qs_0) \sin\} \lambda t}{\text{same denominator}}, \quad (91)$$

where  $q, \lambda, s_0, s_2$  are as in (64), (65). The differentiation shown in the denominator is to be performed upon the function of  $ml$  to which  $\tan ml$  is equated in (89) after reduction to the form of such a function in the way explained; and  $\theta$  depends upon  $Z_0$  thus,

$$\left. \begin{aligned} \tan \theta &= -m^{-1}(K + Sp)Z_0, \\ \sec^2 \theta &= 1 + m^{-2}Z_0^2(K + Sp)^2, \end{aligned} \right\}, \quad \dots \quad (92)$$

which are also functions of  $ml$ . It should be remarked that the terms depending upon solitary roots, occurring in the case  $m^2=0$ , are not represented in (90), (91). They must be carefully attended to when they occur.

#### NOTE A.

An electromagnetic theory of light becomes a necessity, the moment one realizes that it is the same medium that transmits electromagnetic disturbances and those concerned in common radiation. Hence *the* electromagnetic theory of Maxwell, the essential part of which is that the vibrations of light are really electromagnetic vibrations (whatever they may be), and which is an undulatory theory, seems to possess far greater intrinsic probability than *the* undulatory theory, because that is not an electromagnetic theory. Adopting, then, Maxwell's notion, we see that the only difference between the waves in telephony (apart from the distortion and dissipation due to resistance) and light-waves is in the wave-length; and the fact that the speed, as calculated by electromagnetic data, is the same as that of light, furnishes a powerful argument in favour of the extreme relative simplicity of

constitution of the æther, as compared with common matter in bulk. There is observational reason to believe that the sun sometimes causes magnetic disturbances here of the ordinary kind. It is impossible to attribute this to any amount of increased activity of emission of the sun so long as we only think of common radiation. But, bearing in mind the long waves of electromagnetism, and the constant speed, we see that disturbances from the sun may be hundreds or thousands of miles long of one kind (*i. e.* without alternation), and such waves, in passing the earth, would cause magnetic "storms," by inducing currents in the earth's crust and in telegraph-wires. Since common radiation is ascribed to molecules, we must ascribe the great disturbances to movements of large masses of matter.

There is nothing in the abstract electromagnetic theory to indicate whether the electric or the magnetic force is in the plane of polarization, or rather, surface of polarization. But by taking a concrete example, as the reflexion of light at the boundary of transparent dielectrics, we get Fresnel's formula for the ratio of reflected to incident wave, on the assumption that his "displacement" coincides with the electric displacement; and so prove that it is the magnetic flux that is in the plane of polarization.

#### NOTE B.

I give these numerical examples:—

Take a circuit 100 kilom. long, of 4 ohms and  $\frac{1}{4}$  microf. per kilom. and *no* inductance in the first place, and also no leakage in any case. Short-circuit at beginning A and end B. Introduce at A a sinusoidal impressed force, and calculate the amplitude of the current at B by the electrostatic theory. Let the ratio of the full steady current to the amplitude of the sinusoidal current be  $\rho$ , and let the frequency range through 4 octaves, from  $n=1250$  to  $n=20,000$ ; the frequency being  $n \cdot 2\pi$ . The values of  $\rho$  are

1·723,    3·431,    10·49,    58·87,    778.

It is barely credible that any kind of speaking would be possible, owing to the extraordinarily rapid increase of attenuation with the frequency. Little more than murmuring would result.

Now make  $L=2$  (very low indeed).  $L$  being inductance per centim. Calculate by the combined electrostatic and magnetic formulæ. The corresponding figures are

1·567,    2·649,    5·587,    10·496,    16·607.

The change is marvellous. It is only by the preservation of the currents of great frequency that good articulation is possible, and we see that even a very little self-induction immensely improves matters. There is no "dominant" frequency in telephony. What



should be aimed at is to get currents of any frequency reproduced at B in their proper proportions, attenuated to the same extent.

Change L to 5. Results:—

1.437,      2.251,      3.176,      4.169,      4.670.

*Good* telephony is now possible, though much distortion remains. Increase L to 10. Results:—

1.235,      1.510,      1.729,      1.825,      1.854.

This is first class, showing approximation towards a non-distortional circuit. Now this is all done by the self-induction carrying forward the waves undistorted (relatively) and also with much less attenuation.

I should add that I attach no importance to the above figures in point of exactness. The theory is only a first approximation. In order to emphasize the part played by self-induction, I have stated that by sufficiently increasing it (without other change, if this could be possible) we could make the amplitude of current at the end of an Atlantic cable greater than the steady current (by the *quasi-resonance*).

#### NOTE C.

In Sir W. Thomson's article on the "Velocity of Electricity" (Nichols's *Cyclopædia*, 2nd edition, 1860, and Art. lxxxii. of 'Mathematical and Physical Papers,' vol. ii.) is an account of the chief results published up to that date relating to the "velocity" of transmission of electricity, and a very explicit statement, except in some respects as regards inertia, of the theoretical meaning to be attached to this velocity under different circumstances. This article is also strikingly illustrative of the remarkable contrast between Sir W. Thomson's way of looking at things electrical (at least at that time) and Maxwell's views; or perhaps I should say Maxwell's plainly evident views combined with the views which his followers have extracted from that mine of wealth 'Maxwell,' but which do not lie on the surface. [As charity begins at home, I may perhaps illustrate by a personal example the difference between the patent and the latent, in Maxwell. If I should claim (which I do) to have discovered the true method of establishment of current in a wire—that is, the current starting on its boundary, as the result of the initial dielectric wave outside it, followed by diffusion inwards,—I might be told that it was all "in Maxwell." So it is; but entirely latent. And there are many more things in Maxwell which are not yet discovered.] This difference has been the subject of a most moving appeal from Prof. G. F. Fitzgerald, in 'Nature,' about three years since. There really seemed to be substance in that appeal. For it is only a master-mind that can adequately attack the great constructive problem of the æther, and its true relation to matter; and should there be reason to believe

that the master is on the wrong track, the result must be, as Prof. Fitzgerald observed (in effect) disastrous to progress. Now Maxwell's theory and methods have stood the test of time, and showed themselves to be eminently rational and developable.

It is not, however, with the general question that we are here concerned, but with the different kinds of "velocity of electricity." As Sir W. Thomson points out, his electrostatic theory, by ignoring electromagnetic induction, leads to infinite speed of electricity through the wire. Interpreted in terms of Maxwell's theory, this speed is not that of electricity through the wire at all, but of the waves through the dielectric, guided by the wire. It results, then, from the assumption  $\mu=0$ , destroying inertia (not of the electric current, but of the magnetic field), and leaving only forces of elasticity and resistance.

But he also points out another way of getting an infinite speed, when we, in the case of a suspended wire, not of great length, ignore the static charge. This is illustrated by the pushing of incompressible water through an unyielding pipe, constraining the current to be the same in all parts of the circuit. This, in Maxwell's theory, amounts to stopping the elastic displacement in the dielectric, and so making the speed of the wave through it infinite. As, however, the physical actions must be the same, whether a wire be long or short, the assumption being only warrantable for purposes of calculation, I have explained the matter thus. The electromagnetic waves are sent to and fro with such great frequency (owing to the shortness of the line) that only the mean value of the oscillatory  $V$  at any part can be perceived, and this is the final value; at the same time, by reason of current in the negative waves being of the same sign as in the positive, the current  $C$  mounts up by little jumps, which are, however, packed so closely together as to make a practically continuous rise of current in a smooth curve, which is that given by the electromagnetic theory. This curve is of course practically the same all over the circuit, because of the little jumps being imperceptible.

But in any case this speed is not the speed of electricity through the wire, but through the dielectric outside it. Maxwell remarked that we know nothing of the speed of electricity in a wire supporting current; it may be an inch in an hour, or immensely great. This is on the assumption, apparently, that the electric current in a wire really consists in the transfer of electricity through the wire. I have been forced, to make Maxwell's scheme intelligible to myself, to go further, and add that the electricity may be standing still, which is as much as to say that there is no current, in a literal sense, inside a conductor. [The slipping of electrification over the surface of a wire is quite another thing. That is merely the movement of the wave through the dielectric, guided by the wire. It occurs in a non-distortional circuit, owing to the absence of tailing, in the most plainly evident manner.] In other words, take Maxwell's definition of electric current in terms of magnetic

force as a basis, and ignore the imaginary fluid behind it as being a positive hindrance to progress, as soon as one leaves the elementary field of *steady* currents and has to deal with variable states.

The remarks in the text on the subject of the speed of waves in conductors relates to a speed that is not considered in Sir W. Thomson's article. It is the speed of transmission of magnetic disturbances into the wire, in cylindrical waves, which begins at any part of a wire as soon as the primary wave through the dielectric reaches that part. It would be no use trying to make signals through a wire if we had not the outer dielectric to carry the magnetizing and electrizing force to its boundary. The slowness of diffusion in large masses is surprising. Thus a sheet of copper covering the earth, only 1 centim. in thickness, supporting a current whose external field imitates that of the earth, has a time-constant of about a fortnight. If the copper extended to the centre of the earth, the time-constant of the slowest subsiding normal system would be millions of years.

In the article referred to, Sir W. Thomson mentions that Kirchhoff's investigation, introducing electromagnetic induction, led to a velocity of electricity considerably greater than\* that of light, which is so far in accordance with Wheatstone's observation. Now it seems to me that we have here a suggestion of a probable

\* [Note by SIR WILLIAM THOMSON.] In this statement I inadvertently did injustice to Kirchhoff. In the unpublished investigation referred to in the article *Electricity, Velocity of* [Nichols's *Cyclopædia*, second edition, 1860; or my 'Collected Papers,' vol. ii. page 135 (3)], I had found that the ultimate velocity of propagation of electricity in a long insulated wire in air is equal to the number of electrostatic units in the electromagnetic unit; and I had correctly assumed that Kirchhoff's investigation led to the same result. But, owing to the misunderstanding of two electricities or one, referred to in § 317 of my 'Electrostatics and Magnetism,' I imagined Weber's measurement of the number of electrostatic units in the electromagnetic to be  $2 \times 3.1 \times 10^{10}$  centimetres per second, which would give for the ultimate velocity of electricity through a long wire in air twice the velocity of light. In my own investigation, for the submarine cable, I had found the ultimate velocity of electricity to be equal to the number of electrostatic units in the electromagnetic unit divided by  $\sqrt{k}$ ;  $k$  denoting the specific inductive capacity of the gutta-percha. But at that time no one in Germany (scarcely any one out of England) believed in Faraday's "specific inductive capacity of a dielectric."

Kirchhoff himself was perfectly clear on the velocity of electricity in a long insulated wire in air. In his original paper, "Ueber die Bewegung der Electricität in Drähten" (Pogg. Ann. Bd. c. 1857; see pages 146 and 147 of Kirchhoff's Volume of Collected Papers, Leipzig, 1882), he gives it as  $c/\sqrt{2}$ , which is what I then called the number of electrostatic units in the electromagnetic unit; and immediately after this he says, "ihr Werth ist der von 41950 Meilen in einer Sekunde, also sehr nahe gleich der Geschwindigkeit des Lichtes im leeren Raume."

Thus clearly to Kirchhoff belongs the priority of the discovery that the velocity of electricity in a wire insulated in air is very approximately equal to the velocity of light.



explanation of why Sir W. Thomson did not introduce self-induction into his theory. There were presumably more ways than one of doing it, as regards the measure of the electric force of induction. When we follow Maxwell's equations, there is but one way of doing it, which is quite definite, and leads to a speed which cannot possibly exceed that of light, since it is the speed  $(\mu e)^{-\frac{1}{2}}$  through the dielectric, and cannot be sensibly greater than  $3 \times 10^{10}$  centim., though it may be less. Kirchhoff's result is therefore in conflict with Maxwell's statement that the German methods lead to the same results as his. Besides that, Wheatstone's classical result has not been supported by any later results, which are always less than the speed of light, as is to be expected (even in a non-distortional circuit). But a reference to Wheatstone's paper on the subject will show, first, that there was confessedly a good deal of guesswork; and, next, that the repeated doubling of the wire on itself made the experiment, from a modern point of view, of too complex a theory to be examined in detail, and unsuitable as a test.

XVI. *A Theory of Glacier Motion.*  
By R. MOUNTFORD DEELEY, F.G.S.\*

SOME years have now elapsed since the question of glacier motion was brought prominently before the scientific world by the publication of a number of valuable treatises and letters on the glaciers of the Swiss Alps. Each new theory of glacier-motion, as it was given to the world, necessitated further observation of the phenomena presented by glaciers, or gave increased interest to the many interesting properties of ice. In this way a vast amount of information has been collected by numerous trained observers, about which there is little dispute; but to the simple question, "Why does a glacier move?" a great variety of answers would still be given. Indeed, it cannot be said that any substantial progress has been made, for though, as we have seen, a considerable number of theories have been framed, no one of them has yet been anything like generally accepted. Principal J. D. Forbes insisted upon the problem being treated as one of pure mechanics; and to him we largely owe that knowledge of glacier phenomena which Professor Tyndall and others have so ably worked out. Still the old question remains unanswered. We know how the glacier moves, but cannot say why it moves. To add another theory to the already long list may be regarded as tending to make matters

\* Communicated by the Author.



still worse; but I venture to think that, as the heat of former discussion has now dissipated itself, and no theory has succeeded in standing the test of time, another attempt is justifiable. I do not underrate the splendid work done by Rendu, Forbes, Tyndall, Thomson, and many other eminent men; for, as I have previously indicated, to them we owe a series of observations on glacier-motion and structure which will always rank high among the scientific achievements of the present century. Still, the exact details of the process which goes on within a glacier as it slowly changes its form, accommodates itself to the ever-varying shape of its rocky channel, and moves downwards with a differential motion under the action of gravity, is still legitimate matter for speculation.

The theories now in the field are too well known to render any detailed reference to them necessary; I will therefore content myself with stating, as simply as possible, what may be regarded as a purely mechanical explanation of the observed facts.

It should be borne in mind that all the changes of form which a body undergoes, provided no increase or decrease of bulk takes place, may be regarded as due to what is called simple shear. Take a pile of notepaper, or a pack of cards, and displace it a little so that instead of standing vertically it forms an inclined heap. Change of form has taken place without change of bulk. The distortion has been accomplished by the shearing of the pile in planes parallel and coincident with the sheets of paper or card. Instructive examples may be seen in the quires of notepaper piled up in various forms in shop windows. Every change of outline suffered by a glacier, if we disregard melting and the small internal changes of bulk produced by pressure &c., is due to a shear of ice plane over ice plane. The resistance offered to the sliding of the particles of one plane over the particles of another plane is called friction; and the resistance they offer to being torn asunder is called cohesion. Now the rigidity, elasticity, &c., of a substance will depend upon the qualities and values of these two properties. For instance, cast iron breaks when put in tension without pulling out to any extent; for the cohesion of its particles is not sufficient to overcome internal friction, and allow much more than elastic shear to take place before breaking. On the other hand, the cohesion between the particles of wrought iron is so great that it stretches out—that is shears—and becomes heated by the resulting friction.

We are now in a position to understand an experiment

which will be of great value to us further on. Take a plate of steel, say twenty-four inches long, three inches deep, and half an inch thick. Firmly fix one end to a suitable support so that the steel plate shall form a girder with its greatest depth in a vertical position. Then distribute a number of weights along the length of the bar. It at once becomes deflected; that is shear, *elastic shear*, is produced in a manner somewhat analogous to that in the pile of notepaper. We will now drill a row of holes along the plate; and when this has been done, the girder, having been weakened, will be found to have taken a still greater amount of set. Still further increase the set by drilling several rows of holes. So far all the operations have been possible ones; but I must now draw upon the imagination somewhat, and perform operations which cannot be carried out in practice. Take the material removed from the numerous perforations in the plate, and replace it so that the plate becomes whole again. It is evident that though again solid, only that metal which formed part of the original perforated plate is in a state of strain, that filling the holes taking no share of the load. We will again drill a number of holes, this time in the spaces between the older perforations, and another increase will take place in the deflection of the plate. A strain will also be put upon the metal in the first series of holes bored; and, in addition, a greatly increased strain thrown upon what remains of the original plate. By repeating the operation the girder could be deformed to any desired extent, and, if necessary, such a violent strain thrown upon any one point that *local rupture* would ensue. The shear we have produced, and also that which took place in the pile of paper, can now be compared with that taking place in an ice-stream. Place the steel plate horizontally on a map of a glacier drawn to a suitable scale, the end which was used as a support being placed near the edge of the ice, and the other end, which carried the weights, near the centre of the glacier. The differential motion Tyndall so carefully measured will then be found to be similar to, if not congruent with, the shear which the steel plate has undergone. It must be remembered that the ice is a girder of great depth, and the shear surfaces are therefore approximately plane, whereas in the experimental plate they were surfaces of considerable curvature. What is the bearing of the knowledge thus gained upon the question of glacier-motion?

Ice, though hard and brittle, is still an elastic substance. When in mass it undergoes a certain amount of deformation like any other body, by virtue of its own weight and elasticity.

If a mountain-mass could be freed from the attraction of the earth, it would, owing to the elasticity of its component rocks, increase in height by many feet. Every change of atmospheric pressure produces an exceedingly small rise or fall of the earth's surface. The ebb and flow of the tides has a similar effect upon the land in the neighbourhood of the coast-lines; indeed, every substance not absolutely rigid (and absolutely rigid substances do not exist) must, when subjected to any stress whatever, show a corresponding strain or deformation.

A glacier upon whose mass we will suppose gravity has previously had no effect is suddenly put within the sphere of the earth's attraction. It instantly undergoes a change of form. In fact strains are produced in the ice each proportional to the measure of the corresponding stress, and the ice takes a small step in a downward direction, that is in the direction of least resistance. If the glacier rested in a valley with straight smooth sides, the ice would descend bodily and continue to slide until stopped by an angle in its channel or other obstruction. Even on an uneven or crooked bed a certain amount of slide occurs until the bending strains are proportional to the stresses. We therefore have two kinds of motion—one a bodily slide in a downward direction, and another descent due to the differential motion of the ice not in contact with the ground, the resultant motion being proportional to these two components. De Saussure supported a sliding theory; but in a rocky uneven gorge, before continuous sliding can take place, the ice must be capable of continually changing its form to creep over the inequalities of its rocky bed. Of course, when ice is pressed hard against a boss of rock, thawing must go on at a temperature below the ordinary freezing-point, a portion of the water produced recondensing on liberation. A similar effect is obtained by passing a wire over a block of ice and hanging a weight upon it. The wire passes through the mass without splitting it into two portions. That glaciers do slide is capable of absolute proof. Examine the rocky floor over which a glacier has moved. It is frequently beautifully polished, scratched, and grooved. Stones of all sizes have become embedded in the ice, and as it carried them along, the adjacent rocks against which they were pressed underwent considerable abrasion. The polishing and finer markings are effected by the clay, sand, and smaller stones. In this way many of the inequalities of the ground are worn away by glaciers; and the markings give us conclusive proof, not only of the bodily motion of glaciers, but



also of their former existence in areas where they are now absent. Such evidence may be seen scattered over almost the whole of Northern and Central Europe.

We have so far only considered the initial motion of a glacier resulting from its weight and elasticity. As soon as the strains became proportional to the stresses (we shall be sufficiently accurate if we regard the problem as a kinematical one), all movements ceased; and, without the existence of some further reason for motion, the ice would remain motionless in its bed. Generally speaking the strains produced by weight alone would not be sufficient to cause more than local rupture in such a hard material. The distortion so far undergone may be considered as one of elastic shear.

Displace a glacier by an equal bulk of firm clay. The clay, being heavier than the ice and nothing like so capable of resisting shear, ought, on the fracture theory, to move; but in reality it would remain in the valley for a lengthened period without showing any signs of continuous motion. Now, if the weight of the clay is not sufficient to produce shear and give rise to motion, why should gravity act so powerfully upon each particle of ice, a much harder and more cohesive material, and cause the whole to move like a sluggish river? Fracture cannot occur until an amount of general movement has taken place sufficient to throw strains upon some portions greater than the ice at such points can withstand without rupture. What we have, therefore, to explain is how this general movement comes about: fracture and regelation are mere secondary phenomena arising from glacier motion.

Now we have seen how a glacier takes an initial set, or elastic shear, by reason of its weight and elasticity. Every portion of the mass is supporting a certain proportion of the load; and every internal particle of ice which at any time thaws and ceases to be an element of strength gives rise to a further set or movement. On the other hand, when the water in a cavity freezes, it does not begin to afford any support until a strain is thrown upon it by the differential motion of surrounding particles. *Gravity may therefore give rise to a slow but continuous change of form in an elastic substance in the interior of which liquefaction and resolidification are constantly going on.* Changes of bulk (I do not mean change of form or reduction of mass by thawing) do not take place to any extent in a glacier as a whole; what we have to consider is shear, a sliding of particle over particle, producing change of form or distortion, whether internal or external. In an experiment with the steel plate it was proved that by destroying at numerous points the support afforded by the metal,



then restoring the continuity of the mass, and repeating the operation, any amount of deformity could be produced. It therefore follows that if it can be shown that constant liquefaction and resolidification is going on within a glacier, we have all the essential requisites for producing glacier-motion.

It would be difficult to say which is the most interesting of the numerous properties of water in its liquid and solid states. We are in the habit of considering its freezing-point as being a fixed temperature; but Professor W. Thomson proved experimentally that the melting-point of ice is lowered by about the seventy-fifth part of a degree Fahrenheit for every additional atmosphere of pressure. That is, ice under a pressure of about eleven hundred pounds per square inch melts at  $31^{\circ}$  instead of  $32^{\circ}$  Fahr. Hold a slab of lake-ice in the sun's rays. The heat attacks the ice in its interior and produces liquid flowers of great beauty. Watch a flower as it develops! Suddenly a clink is heard, and a bright vacuous spot appears like a silver bead in the centre of the flower. Before the spot formed, the water in the cavity was in a state of tension, and the clink heard was the vibration produced by the tension overcoming the cohesion of the particles of water, and causing rupture. A condition somewhat similar to that of tension, or negative pressure, exists in water when freed from air and heated in a bath of linseed-oil and oil of cloves. In this way it has been raised, while under a pressure of one atmosphere only, to a temperature of  $356^{\circ}$  F. Now  $356^{\circ}$  F. would indicate a cohesive force equal to nearly ten atmospheres. The negative pressure of the water formed by the internal melting of ice, in the absence of air-bubbles, probably exceeds this amount. If positive pressure lowers the freezing-point, negative pressure should raise it. From this it follows that the water in a liquid flower or glacier cavity is surrounded by and in contact with ice sufficiently cold to freeze it directly the source of heat which produced liquefaction is removed. Professor Tyndall placed some lake-ice containing cavities filled with water and air in a warm-water bath. As soon as a cavity was reached by the melting of its walls the inclosed air-bubble at once shrank to a small proportion of its original volume, showing that even in this case the freezing-point of the water was above that of the surrounding ice. Other experiments which it is needless to relate have been made confirming this fact.

During fine sunny weather, or even with warm rain, glaciers have been shown to move more rapidly than when the weather is cold and snowy; and if the glacier could be cooled several degrees below the freezing-point, and shaded

from radiant heat, all motion would cease; for experiment tells us that it is only when at or about the freezing-point that ice is what Principal Forbes called viscous. It is interesting to note that those particles of ice which have to carry the greatest load, that is, are in a state of compression, are the most likely to be the first melted; a fact which not only accounts for the inability of ice to extend much when in tension, but increases the efficacy of *liquefaction and recongelation* to produce glacier-motion.

I will not attempt to refer to the bearing of this theory upon ribboned structure and the many other interesting peculiarities of glacier ice, the present opportunity not being convenient for their detailed treatment.

## XVII. *Intelligence and Miscellaneous Articles.*

### ON THE INFLUENCE OF LIGHT UPON THE ELECTRIC DISCHARGE.

BY E. WIEDEMANN AND H. EBERT.

PROF. H. HERTZ\* in a recent paper, "On the Influence of Ultra-Violet Light upon the Electric Discharge," has described a series of experiments which show that light-rays, and, according to Prof Hertz, exclusively the extreme ultra-violet rays, exert a certain influence upon the passage of electricity in a spark-discharge. It remained uncertain whether this influence was of a primary nature, or whether the passage of the discharges was effected by secondary impulses produced by the action of the light. In order to obtain a nearer insight into the conditions and nature of the phenomenon, we have, in the first place, repeated Hertz's experiment, and always with complete success: then we have further varied the conditions of the experiment, and have thus obtained the following results. As source of light we nearly always employed an electric arc-lamp; in particular cases burning magnesium.

(1) The phenomenon is seen with the greatest regularity not only with the discharge of an induction-coil, but also when a Holtz machine is used, that is, with statical electricity, if the discharge upon which the light acts is contained in a secondary circuit of a Holtz machine, the spark-length of the primary circuit being varied by means of a spark-micrometer. Illumination of the discharge produces a lowering of the potential necessary for the passage of the electricity.

(2) In order to study the influence of the light during the passage of electricity in the passive discharge, one pole of the machine was connected with the earth; the electricity accumulated upon the

other pole passed first through the spark-length, then through a discharge-tube of the usual form, or a telephone wrapped with india-rubber, and then to earth. The result was that not only the rhythm of the discharge was entirely different (as shown by the altered note in the telephone), but the whole character of the discharge was altered, as the appearance of the Geissler-tube showed without difficulty. In place of the irregular discharge, upon illumination a steady discharge occurred (also clearly observed directly in the spark length); but all this occurred *only with the negative discharge* through the spark-length. With the passage of positive electricity, there is no difference between illumination and non-illumination. In order to obtain the phenomena, sharply disturbing reflexions of the light from one ball upon the other must be avoided. In the same way the phenomena may be disturbed by the induction of the electricity accumulated upon the balls. If we avoid these disturbances by suitable arrangements, then, with atmospheric pressure, upon illumination and passage of negative electricity, we hear a note, and upon non-illumination a noise veiling a much deeper note. With positive electricity there is no difference to be observed. The change which the discharge itself suffers is very striking. When the light does not reach it, there are irregular paths in which the electricity passes which change their form and place so often that, in consequence of the persistence of the image upon the retina, there are several to be seen at the same time; as soon as the first ray of light reaches the place at which the negative electricity enters the spark-distance, all these paths unite into one, in which the electricity passes perfectly steadily and uniformly without noise as at first. At both balls it is at right angles to their surface.

(3) The intensity of this action of the rays upon the negative electricity is essentially dependent upon the pressure of the gas in which the experiment is made; with certain moderate pressures and distances between the balls, it reaches its maximum, from which it diminishes rapidly both with increase and with decrease of pressures.

(4) Further, the mode of illumination plays an important part; it is essentially only one point of the electric light (which was the part exclusively employed in the later experiments), and that the hottest part of the carbons, which is especially concerned in producing the phenomenon; the rays therefrom must accurately strike the point where the discharge strikes the negative ball; for the reason already given, it is advantageous to shade the positive ball.

(5) The phenomenon is seen both in dry and in moist atmospheric air, in dry and moist hydrogen as well as in an atmosphere of carbonic acid; in the latter gas remarkably clear and sharp. The maximum intensity of action occurs in different gases at different pressures; this has apparently an intimate connexion with the development of the cathode-rays.

(6) The phenomenon is certainly not exclusively associated with the ultra-violet rays; this is seen most clearly in the experi-

ments with moist carbonic acid ; here it is essentially rays which belong to the visible part of the spectrum which show themselves active. For if, to begin with, the ultra-violet light be shut off by means of a glass plate placed before the lamp, we yet observe the most marked difference if, by means of a board, a red or a green glass, the more refrangible visible rays are taken away ; blue glass allowed the active rays to pass to a large extent. That it cannot be the heat-rays which bring about the observed difference, is seen from the fact that a plate of alum or a glass trough filled with alum-solution did not affect the action. It is to be remembered that the kathode-rays are of different colours in carbonic acid and in hydrogen.

(7) From these experiments, and particularly from the fact that it is exclusively the negative electricity which is influenced by the light, we come to the conclusion that the above phenomena are most intimately related to the production of the kathode-rays. The conditions favourable to the development of the synchronously-vibrating kathode-rays are facilitated by the light falling on the electrodes, since their wave-length is different in different gases. Different gases must be specially sensitive to the action of different regions of the spectrum of different wave-length. The development of the kathode-rays is further dependent, in a manner conditioned by the individual peculiarity of the gas, upon the pressure ; so that the magnitude of the influence which light striking the electrodes can exert must also be dependent upon the pressure. The phenomenon we are considering may then probably be regarded as a kind of resonance-phenomenon, somewhat similar to the bringing about of an explosion by the detonation of certain definite substances only. The kathode-rays, as it were, make the way clear for the succeeding discharge.—*Proc. of the Phys.-Med. Inst. of Erlangen.*

---

SIR WILLIAM THOMSON'S PAPER "ON THE APPLICATION OF THE DECI-AMPERE OR CENTI-AMPERE BALANCE TO THE DETERMINATION OF THE ELECTROMOTIVE FORCES OF VOLTAIC CELLS."

On account of a numerical error in this paper (see *Philosophical Magazine* for December 1887), pointed out by Mr. Desmond Fitzgerald in a communication to the 'Electrical Engineer' for January 6, 1888, referring to Mr. Gray's measurement of November of the electromotive force of Clark cells constructed by Mr. Bottomley last March, substitute for the last four lines of the article the following:—"correcting to 15° C. we obtain 1.4346 Rayleigh volts at that temperature. This result is interesting as showing a difference of less than  $\frac{1}{30}$  per cent. from that obtained by Lord Rayleigh for similar cells, which was 1.435 at 15° C."



THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

---

[FIFTH SERIES.]

---

MARCH 1888.

---

XVIII. *On a Law of Distribution of Molecular Velocities amongst the Molecules of a Fluid.* By JOHN BUCHANAN, B.Sc.\*

IT is known† that if a mechanical system at rest be set in motion by impulses, the kinetic energy of the system is a maximum or a minimum.

The kinetic energy is always a maximum when the impulses are given, and when different motions possible under the conditions of the system, and fulfilling the law of energy, are considered. It is a minimum if the system be started from rest by impulses applied only at places where the velocities to be produced are specified.

The present paper has for its object the introduction of these theorems into the Kinetic Theory of Gases, and into questions whose mathematical treatment may be of a similar nature.

Many years ago Clerk-Maxwell discovered his law relating to the most probable manner of distribution of the molecular velocities amongst the molecules of a quantity of gas whose centre of gravity is at rest, or is moving uniformly in a straight line. I propose to show that a law of the same form holds for the distribution of the molecular velocities in a fluid whose molar kinetic energy fulfils a maximum, or a minimum,

\* Communicated by the Author.

† See Thomson and Tait's 'Natural Philosophy,' vol. i. part 1. §§ 311 and 312.

or a maximum-minimum condition. It is of course assumed that the fluid has a "coarse-grained" structure.

In the proof given below, the important assumption is also made that the molecular energy of the fluid is wholly due to the linear motion of the molecules; or, rather, that if any portion of this energy exists in the forms of molecular rotation and vibration, this portion remains constant in value, and may therefore be left out of account in what follows.

### Proof\*.

By the term "element of volume at a point" is to be understood a very small volume enclosing the point. This elementary volume is supposed to be extremely small compared with the whole volume occupied by the fluid, but very large compared with the average space occupied by a molecule of the fluid.

The molar velocity of such an element of volume of the fluid may be defined in precisely such terms as those in which Clerk-Maxwell defines the velocity of a gas†:—If we determine the motion of the centre of gravity of all the molecules within the element of volume, then the molar velocity of the elementary volume of fluid may be defined as the velocity of the centre of gravity of all the molecules within that region.

In an element of volume at a point P let there be N molecules. Let us take three rectangular axes of reference, and let us denote the components of the linear velocity of the  $n$ th molecule by  $u_n, v_n, w_n$ , and its mass by  $m_n$ .

A summation extended throughout the elementary volume we will denote by  $\Sigma$ , whilst the sign of integration will indicate a summation extended throughout the whole mass of fluid.

Then we can write

$$E = \frac{1}{2} \int \Sigma (u_n^2 + v_n^2 + w_n^2) m_n \dots \dots \dots (1)$$

$$\left. \begin{aligned} a \Sigma m_n &= \Sigma m_n u_n, \\ b \Sigma m_n &= \Sigma m_n v_n, \\ c \Sigma m_n &= \Sigma m_n w_n; \end{aligned} \right\} \dots \dots \dots (2)$$

where E denotes the molecular kinetic energy of the whole mass of fluid, and  $a, b, c$  express the components of the molar velocity of the element of volume at the point P.

The fluid being supposed to have been set in motion initially

\* The method of proof is taken from Meyer's *Kinetische Theorie der Gase*, see § 118 *et seq.*

† 'Theory of Heat,' p. 311.

and then left to itself, if we assume that there is no further change of energy due to outside influences, then  $E$  in (1) is constant. Hence

$$\delta E = 0 = \int \sum (u_n \delta u_n + v_n \delta v_n + w_n \delta w_n) m_n. \quad (3)$$

Also, if we imagine small variations of the velocities that enter into equations (2) to be made consistent with the kinematic conditions of the fluid, we may write

$$\int \delta a \cdot \sum m_n = \int \sum m_n \cdot \delta u_n, \quad (4)$$

$$\int \delta b \cdot \sum m_n = \int \sum m_n \cdot \delta v_n, \quad (5)$$

$$\int \delta c \cdot \sum m_n = \int \sum m_n \cdot \delta w_n. \quad (6)$$

We can now proceed to state the condition that the distribution of the molecular velocities is the most probable.

The probability that for a molecule taken at random the component velocities are  $u, v, w$ , will be some function of  $u, v, w$ , and may be expressed by  $F(u, v, w)$ .

The probability that a number of *independent* events may all happen is the product of the separate probabilities. Hence the probability that the component velocities of the molecules composing the whole mass of fluid—considered as mutually independent—may be respectively

$$(u_1, v_1, w_1), \quad (u_2, v_2, w_2), \quad (u_3, v_3, w_3), \dots$$

is given by

$$F(u_1, v_1, w_1) \quad F(u_2, v_2, w_2) \quad F(u_3, v_3, w_3) \dots$$

or, shortly,

$$F_1 F_2 F_3 \dots \quad (7)$$

Expressing next the condition that the assumed distribution of velocities is the most probable, we have, from (7),

$$\begin{aligned} 0 = & \left( \frac{dF_1}{du_1} \cdot \delta u_1 + \frac{dF_1}{dv_1} \cdot \delta v_1 + \frac{dF_1}{dw_1} \cdot \delta w_1 \right) \cdot F_2 F_3 F_4 \dots \\ & + \left( \frac{dF_2}{du_2} \cdot \delta u_2 + \frac{dF_2}{dv_2} \cdot \delta v_2 + \frac{dF_2}{dw_2} \cdot \delta w_2 \right) \cdot F_1 F_3 F_4 \dots \\ & + \dots \end{aligned}$$

Dividing the terms by the continued product  $F_1 F_2 F_3 \dots$  we may write the result under the form

$$0 = \int \sum \frac{1}{F_n} \left( \frac{dF_n}{du_n} \cdot \delta u_n + \frac{dF_n}{dv_n} \cdot \delta v_n + \frac{dF_n}{dw_n} \cdot \delta w_n \right) \cdot \dots \quad (8)$$

We have therefore five equations, viz. (3), (4), (5), (6), and

(8), to be satisfied simultaneously. From these equations it is possible to eliminate four of the variations: the remaining variations are quite arbitrary. For the elimination, multiply the equations (3), (4), (5), and (6) respectively by  $2k$ ,  $-2k\alpha$ ,  $-2k\beta$ ,  $-2k\gamma$ , as indeterminate coefficients, and add the results to equation (8). This gives

$$\begin{aligned} \int \Sigma \left\{ \left( \frac{1}{F_n} \cdot \frac{dF_n}{du_n} + 2km_n(u_n - \alpha) \right) \delta u_n + \left( \frac{1}{F_n} \cdot \frac{dF_n}{dv_n} + 2km_n(v_n - \beta) \right) \delta v_n \right. \\ \left. + \left( \frac{1}{F_n} \cdot \frac{dF_n}{dw_n} + 2km_n(w_n - \gamma) \right) \cdot \delta w_n \right\} \\ = -2k \int (\alpha \delta a + \beta \delta b + \gamma \delta c) \Sigma m_n. \quad . \quad . \quad . \quad . \quad . \quad (9) \end{aligned}$$

Let the indeterminate multipliers be so chosen—being arbitrary constants—that the right-hand member of equation (9) vanishes. At the same time they can be chosen so as to make each of the multipliers of the arbitrary variations on the left-hand side of the equation equal to zero. Hence

$$\int (\alpha \delta a + \beta \delta b + \gamma \delta c) \Sigma m_n = 0. \quad . \quad . \quad . \quad (10)$$

For the other quantities, we may write, as a typical set of terms,

$$\frac{1}{F} \cdot \frac{dF}{du} + 2km(u - \alpha) = 0, \quad . \quad . \quad . \quad . \quad (11)$$

$$\frac{1}{F} \cdot \frac{dF}{dv} + 2km(v - \beta) = 0, \quad . \quad . \quad . \quad . \quad (12)$$

$$\frac{1}{F} \cdot \frac{dF}{dw} + 2km(w - \gamma) = 0. \quad . \quad . \quad . \quad . \quad (13)$$

These last give, by integration,

$$F(u, v, w) = C e^{-km((u-\alpha)^2 + (v-\beta)^2 + (w-\gamma)^2)}.$$

Thus the probability that the component velocities lie between  $u$  and  $u + du$ ,  $v$  and  $v + dv$ ,  $w$  and  $w + dw$  respectively may be written

$$F(u, v, w) = B^3 \cdot e^{-km((u-\alpha)^2 + (v-\beta)^2 + (w-\gamma)^2)} du dv dw. \quad . \quad (14)$$

The probabilities for the existence of the components are therefore respectively

$$B e^{-km(u-\alpha)^2} du,$$

$$B e^{-km(v-\beta)^2} dv,$$

$$B e^{-km(w-\gamma)^2} dw.$$

And since each component velocity must lie somewhere between  $-\infty$  and  $+\infty$ , we have



$$1 = B \int_{-\infty}^{\infty} e^{-km(u-\alpha)^2} \cdot du = B \int_{-\infty}^{\infty} e^{-km(v-\beta)^2} \cdot dv = B \int_{-\infty}^{\infty} e^{-km(w-\gamma)^2} \cdot dw.$$

Hence

$$1 = B \sqrt{\frac{\pi}{km}} \cdot \dots \dots \dots (15)$$

Again, since  $\alpha, b, c$  are the mean values of the component velocities,

$$a = B \int_{-\infty}^{\infty} e^{-km(u-\alpha)^2} \cdot u \, du,$$

$$b = B \int_{-\infty}^{\infty} e^{-km(v-\beta)^2} \cdot v \, dv,$$

$$c = B \int_{-\infty}^{\infty} e^{-km(w-\gamma)^2} \cdot w \, dw.$$

These give, by help of (15),

$$\alpha = a ; \quad \beta = b ; \quad \gamma = c.$$

Equation (14) may therefore be written

$$F(u, v, w) = \left(\frac{km}{\pi}\right)^{\frac{3}{2}} e^{-km((u-a)^2 + (v-b)^2 + (w-c)^2)} \cdot du \, dv \, dw. \quad (16)$$

Let us return now to equation (10), and substitute in it the value of  $\alpha, \beta$ , and  $\gamma$ . Then

$$\int (a \, \delta a + b \, \delta b + c \, \delta c) \Sigma m_n = 0.$$

Or

$$\delta T = 0 ; \quad \dots \dots \dots (17)$$

where

$$T = \frac{1}{2} \int (a^2 + b^2 + c^2) \Sigma m_n.$$

The quantity  $T$  is evidently the molar kinetic energy of the fluid. The condition  $\delta T = 0$  expresses that this kinetic energy is a maximum, or a minimum, or a maximum-minimum.

Hence the theorem:—If the molar kinetic energy of a fluid fulfils a maximum, or a minimum, or a maximum-minimum condition, then the distribution of molecular (linear) velocities follows a law of the same form as that of Maxwell for gases.

Gordon's College, Aberdeen,  
January 1888.

XIX. *On the Price of the Factor of Safety in the Materials for Lightning-rods.* By Professor SILVANUS P. THOMPSON, D.Sc.\*

IT is possible to determine in an absolute manner what metal is best for securing safety by a system of lightning-conductors, for a given prime cost. In the calculations that follow it is assumed that the duration of the lightning-discharge is so brief that there is no time for appreciable radiation or convection of heat from the surface of the conductor, or for conduction into other bodies ; and that the conductor is devoid of self-induction.

Safety is dependent, other things being equal, upon the difficulty of fusion of the conductor. The difficulty of fusion of the conductor varies directly as the fusing-point of the material (or, more strictly, as the difference between this temperature and that of the surrounding air, assumed here to be  $15^{\circ}$  C.), and inversely as the temperature-rise occasioned in it by the discharge of electricity, the amount of which is supposed to be given. The temperature-rise varies inversely as the specific thermal capacity of the material, and directly as the heat developed in it. The heat caused by a given discharge of electricity varies directly as the total electric resistance of the conductor. The total electric resistance of the conductor varies directly as the specific electric resistance per unit cube of volume of the material of the conductor, also directly as its length, and inversely as its area of cross section (supposed uniform throughout). The area of section varies directly as the volume and inversely as the length (supposed given). The volume varies inversely as the density of the material and directly as its mass. The mass of the conductor varies directly as the total cost, and inversely as the cost per unit of mass.

Hence, writing  $f$  for the temperature of fusion above that of the surrounding air (assumed at  $15^{\circ}$  C.),  $s$  for the specific thermal capacity,  $\rho$  for the specific electric resistance,  $l$  for the given length,  $d$  for the density,  $k$  for the cost, in pence, per pound of the material of the conductor, it at once follows that

$$\text{Safety varies as total cost} \times \frac{f \times s}{\rho \times d \times l^2 \times k}.$$

Hence, as the total cost and the length are supposed to be

\* Communicated by the Physical Society : read January 28, 1888.

given, we have

$$\text{Factor of safety for equal total cost} = \frac{f \times s}{\rho \times d \times k}.$$

The only metals about which it is necessary to inquire are copper, silver, iron, lead, platinum, and tin. In the following table the values of  $f$  are deduced from the figures of Violle and of Rudberg. The values of  $k$  are deduced from the market-prices in London on December 7, 1887: viz. copper, £75 per ton; silver, 4s. per troy ounce; iron (rod) £5 10s. per ton; lead, £16 10s. per ton; platinum, 32s. per troy ounce; tin, £170 per ton. The figures given in column 6 for the factor of safety for equal cost are calculated by the foregoing formula multiplied by  $10^4$ . The figures in the seventh column, giving the total cost of equal safety, so far as prime cost of material is concerned, are of course inversely proportional to the figures in column 6.

	$f$ .	$s$ .	$\rho$ .	$d$ .	$k$ .	Factor of safety.	Total cost for equal safety.
Copper ...	1039	0.0949	1615	8.94	8.045	9.488	£100
Silver .....	939	0.0570	1609	10.51	699.84	0.045	£18,770
Iron .....	1585	0.1138	9827	7.79	0.589	39.97	£21 9s.
Lead .....	307	0.0314	19847	11.35	1.769	0.242	£3508
Platinum..	1760	0.0325	9158	21.36	5598.7	0.00052	£1,621,600
Tin .....	213.5	0.0548	13360	7.29	18.22	0.6593	£1287

The prime cost of copper is known to be greater than that of iron for equal conductivity; and reckoning on this basis, irrespective of difficulty of fusion, it is stated in the Report of the Lightning-rod Conference to be 50 per cent. dearer. If that were all, then doubtless the small saving in using iron would be more than counterbalanced by the attendant disadvantages of deterioration by rust, and the like. But when the higher fusion-point, the greater capacity for heat, and the lesser density of iron are taken into account, the superiority of iron, for equal total prime cost, becomes so marked as to once more raise the question whether it may not be expedient to return to the original practice of Franklin, and use lightning-rods of iron instead of those of copper.

London, December 12, 1887.

XX. *On some Questions in the Kinetic Theory of Gases.*  
*Reply to Prof. Boltzmann.*

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

AS you have translated Prof. Boltzmann's voluminous attack, I have to request you to publish my reply, communicated to the Royal Society of Edinburgh on January 30th. I have compressed it as far as I can, for I fear to weary your readers by lengthened discussions of a somewhat personal character.

With reference to Mr. Burbury's note, I will only say for the present that I pretend to no "authority," having approached this subject simply as a student; and that it is with the statements of Clerk-Maxwell and Clausius that I hold him to be at variance.

Yours truly,

P. G. TAIT.

Hearing, again by accident, that Professor Boltzmann has in the Vienna *Sitzungsberichte* published a new attack on my papers about the *Kinetic Theory*, I at once ordered a copy, which has at length arrived. As my papers appeared in our 'Transactions,' I think my answer to this fresh attack should be communicated in the first place to this Society. The time I can spare for such work at this period of the year is very scant, and Prof. Boltzmann has raised a multitude of questions. I will take them in order. But I must commence by saying, with reference to Prof. Boltzmann's peculiar remarks on my behaviour as a critic, that, while leaving them to the judgment of readers, I shall have to bring before the same readers several instances in which Prof. Boltzmann has completely misstated the contents or the objects of my papers. This is not a new departure. In his first attack on me he said that I had nowhere stated that my investigations were confined to hard spherical particles; whereas I had been particularly explicit on that very point. But fresh cases of a similar character abound in this new attack.

*First.* There runs through this paper an undercurrent, at least, of accusation against me for putting forward my results as new, and thus ignoring the work of others. I had no such intention, and I do not think anything I have said can bear such a construction. My knowledge of the later history of the subject is no doubt now considerably greater than it was about two years ago, when, at Sir William Thomson's



request, I undertook an examination of Clerk-Maxwell's first proof of his own Theorem. But it is still of a very fragmentary character. I had, years ago, read papers by Maxwell and by Clausius, and had glanced at the treatises of O. E. Meyer and Watson. I had also made a collection of various papers by Prof. Boltzmann. But I found that, without much expenditure of time and labour, it would be impossible to master the contents of the three last-named works, mainly because the methods employed seemed to me altogether unnecessarily intricate. [I have already stated the impression produced on me by such of Prof. Boltzmann's papers as I have tried to read, and I need not recur to it.] I therefore set to work for myself, having certain definite asserted results in view, but little knowledge of the processes which their discoverers or propounders had used. After obtaining a demonstration of Clerk-Maxwell's Theorem, I was led to pursue my investigations into other matters, such as the rate of restoration of the special state, the size of molecules, &c. I brought before the Society such of these investigations as I had more fully developed; and I hope to communicate others. One object which I tried to keep constantly in view was to make my papers at least *easily intelligible*. Intelligibility is not too common a characteristic of papers or treatises on this subject. But if I have succeeded in putting some parts of the *Foundations* of the Kinetic Theory (for to these alone do my papers profess to extend) in a form which renders them easily apprehended, I shall have done a real service to students of Physical Science. The other object at which I aimed was, of course, the verification of Maxwell's Theorem; and of the extension of it (to all degrees of freedom of complex molecules) which was made by Prof. Boltzmann. Sir William Thomson and myself were, in fact, called to the question by the discrepancies between the observed behaviour of gases and the behaviour which Prof. Boltzmann's Theorem would have led us to expect. To test this excessively general theorem, I determined to examine certain special cases, and (that these might be, however imperfectly, represented by systems of free particles) it was necessary to assume want of freedom for collision, though confessedly as *one* step only. I could not, of course, in this way put limits on the excursions or the admissible speeds for different degrees of freedom.

*Second.* While examining, and seeking to improve, the proof which Clerk-Maxwell originally gave of his Theorem, I found it impossible to begin without the assumption of a certain regularity of distribution of masses and velocities; and of course I sought how to *justify* such an assumption. I

was thus led to believe that collisions, not merely of particles of the two kinds with one another but among those of each kind, are absolutely necessary for this justification. Then I saw that, in complex molecules, perfect freedom of collisions of all kinds of "degrees of freedom" could not possibly be secured, and that this might, in part at least, account for the discrepance between Prof. Boltzmann's Theorem and the observed behaviour of gases. I saw also that, for the truth even of Maxwell's Theorem, it was necessary that neither of the two gases should be in an overwhelming majority. Thus these two things, which Prof. Boltzmann now speaks of as "physically less important," are from my point of view vital to the general truth of his Theorem.

Prof. Boltzmann commences his recent paper by citing a "general equation" from the Philosophical Magazine of April 1887; and of it he says:—

"Bei Ableitung dieser Gleichung habe ich dort im Übrigen genau dieselben Voraussetzungen zu Grunde gelegt, welche auch Herr Tait machte, nur dass ich über die relative Grösse der Durchmesser  $\lambda$  und  $\Lambda$  der Moleküle beider Gase, sowie über den Grössenwerth des Verhältnisses  $N_1 : N_2$  nicht die mindeste Annahme gemacht habe."

This is so far from being the case, that it was precisely his *assumptions*, and not his proof, which I disputed. My remark was:—

"I think it will be allowed that Prof. Boltzmann's assumptions, which (it is easy to see) practically beg the whole question, are themselves inadmissible, *except as consequences of the mutual impacts of the particles in each of the two systems separately.*"

Of course, with his assumptions, Prof. Boltzmann obtains the desired result:—having in them virtually begged the question. He now blames me for not having said a word in refutation of his proof, for I had professed my willingness to allow its accuracy without even reading it. There was no discourtesy in that remark:—nothing but a cheerful admission that, in the hands of Prof. Boltzmann, such premises could not fail to give the result sought. My comments were in fact *necessarily* confined to the assumptions. For, as I could not admit them, the proof founded on them had no interest for me. Professor Boltzmann assumed that two sets of particles, *even if they have no internal collisions*, will by their mutual collisions arrive at a state of uniform distribution in space, and of average behaviour alike in all directions. This may possibly be true, but it is certainly very far from being axiomatic, and thus demands strict proof before it can be lawfully

used as a basis for further argument. In quoting my remarks on this point Prof. Boltzmann very significantly puts an "&c." in place of the following words:—"it is specially to be noted that this is a question of *effective diameters* only and not of masses:—so that those particles which are virtually free from the self-regulating power of mutual collisions, and therefore form a disturbing element, may be much more massive than the others." It was of this preliminary matter, of course, that I spoke when I wrote the following sentence, which seems to have annoyed Prof. Boltzmann:—

"I have not yet seen any attempt to prove that two sets of particles, which have no internal collisions, will by their mutual collisions tend to the state assumed by Prof. Boltzmann."

I think it probable that Prof. Boltzmann has not fully apprehended the meaning of the word "assumed" in this sentence. Otherwise I cannot understand why he is annoyed because I took his proof for granted.

In taking leave, for the time, of this special question, I need scarcely do more, and I cannot do less, than reaffirm the assertion just quoted:—while adding the remark that this is very far from being my sole objection to Prof. Boltzmann's very general Theorem. In fact Professors Burnside\* and J. J. Thomson† have quite recently advanced other serious objections. Prof. Boltzmann's Theorem, in a word, is not yet demonstrated.

*Third.* As to the questions of viscosity and heat-conduction; my investigations were expressly made on the assumption that change of permeability, due to motion, was negligible. When I found that I had obtained in a very simple way certain characteristic results of Clerk-Maxwell and of Clausius respectively, I was satisfied with the approximation I had made. Prof. Boltzmann does not allude to the fact that my investigation was distinctly stated to be an approximate one only, and that the additional consideration he now adduces had been before me and had been rejected (rightly or wrongly) for reasons given. I said:—

"Strictly speaking, the exponent should have had an additional term . . . . See the remarks in § 39 below."

And, in the § 39 thus pointedly referred to, one of the remarks in question is:—

"We neglect, however, as insensible the difference between the absorption due to *slowly* moving layers and that due to the same when stationary."

And, in fact, the result which I gave for the viscosity (and

\* Trans. Roy. Soc. Edin. 1887.

† Phil. Trans. 1887.



which Professor Boltzmann, without doubt justly, claims as his own) is correct under the conditions by which I restricted my investigation. The introduction of the consideration of change of permeability due to the shearing motion involves an alteration of about eleven or twelve per cent. only in this *avowedly approximate* result. Of this I have assured myself by a rough calculation, and I will work it out more fully when I have leisure. It seems that I have missed this in looking over Meyer's book, and, according to Prof. Boltzmann, all investigators except Meyer have fallen into the same trap. Meanwhile the calculation with which Prof. Boltzmann has furnished me gives an excellent example of his style, for it is altogether unnecessarily tedious. And it seems to contain two gigantic errors which, however, compensate one another. For his integrand contains the factor  $\epsilon^{-x/p}$ . Here  $f$  is a signless quantity, and the limits show that  $x$  is always positive and  $p$  always negative. As written, therefore, the integral is infinite, though in the result it is made to come out finite. The object of the paragraphs 1 and 2, which immediately follow, is unintelligible to me. The former seems to suggest the use of an unsound method; the latter has no discoverable bearing on anything that I have written. Prof. Boltzmann has also afforded an idea of the value which he himself attaches to the terrific array of symbols in the 95 pages of his 1881 paper (to which he refers me) by now allowing that he is not prepared to assert that any one of three determinations of the coefficient of viscosity which he quotes (mine, or rather his own, being among them) is to be preferred to the others!

*Fourth.* Prof. Boltzmann refers to my remarks on Mr. Burbury's assertion that a single particle, with which they can collide, would reduce to the special state a group of non-colliding particles. Prof. Boltzmann signified his belief in the truth of this proposition; and in answer I showed that (were it true) æons would be required for the process, even if that were limited to a single cubic inch of gas. He now calls this an "entirely new question" and will not "prolong the controversy by its discussion." I do not see that, so far at least as the "controversy" is concerned, it is any newer than the rest. It is contained in the first instalment of his attack. Why, then, should he now desert it? But Prof. Boltzmann, in thus leaving the subject, takes a step well calculated to prolong the discussion, for he represents me as speaking of the instantaneous reversal of the motions of *all* the particles, whereas my argument was specially based on *the reversal of the motion of the single stranger alone*, a con-



tingency which might possibly occur by collision even with a particle of the gas, certainly by collision with the containing vessel. There is a common proverb, "All roads lead to Rome." It seems it ought now to be amended by the addition, "whether you go backwards or forwards along them."

*Fifth.* As to my proof (so designated) of the Maxwell Law of distribution of velocities :—I have already explained that this part of my paper was a mere introductory sketch, intended to make into a connected whole a series of detached investigations, and therefore contained no detailed and formal proofs whatever. Maxwell's result as to the error-law distribution of velocities, being universally accepted, was thus discussed in the briefest manner possible. I said also that a detailed proof can be given on the lines of § 21 of my paper. Prof. Boltzmann\* at first accused me of reasoning in a *circulus vitiosus*, and went the extreme length of asserting that the independence of velocities in different directions can do no more than prove the density (in the velocity space diagram) to be dependent on the radius vector only. Now, when I have taken the trouble to point out briefly and without detail what I meant by the statements he misunderstood, he says I have admitted that my proof is defective ! For my own part, I see no strong reason wholly to reject even the first proof given by Maxwell ; and it must be observed that although its author said (in 1866) that it depends on an assumption which "may appear precarious," this did not necessarily imply that it appeared to *himself* to be precarious. The question really at issue was raised in a very clear form by Prof. Newcomb, who was the earliest to take exception to my first sketch of a proof. He remarked that it seemed to him to possess too much of a geometrical character (*i. e.* to prove a physical statement by mere space-reasoning), while Maxwell's seemed to involve an unauthorized application of the Theory of Probabilities. In consequence of this objection I examined the question from a great many points of view, but I still think my original statement correct. What I said was "*But the argument above shows further, that this density must be expressible in the form*

$$f(x)f(y)f(z)$$

whatever rectangular axes be chosen passing through the origin." In my second paper I said (in explanation of this

\* This addition to Prof. Boltzmann's first attack on me seems to have appeared in the *Phil. Mag.* alone. It is not in either of the German copies in my possession (for one of which I am indebted to the author), nor do I find it in the *Sitzungsberichte* of the Vienna Academy.

to Prof. Boltzmann) that the behaviour parallel to  $y$  and  $z$  (though not the number) of particles whose velocity components are from  $x$  to  $x+dx$ , must *obviously* be independent of  $x$ , so that the density of "ends" in the velocity-space diagram is of the form  $f'(x) \cdot F(y, z)$ . The word *I* have underlined may be very easily justified. No collisions count, except those in which the line of centres is practically perpendicular to  $x$  (for the others each dismiss a particle from the minority; and its place is instantly supplied by another, which behaves exactly as the first did), and therefore the component of the relative speed *involved in the collisions which we require to consider* depends wholly on  $y$  and  $z$  motions. Also, for the same reason, the frequency of collisions of various kinds (so far as  $x$  is concerned) does not come into question. Thus the  $y$  and  $z$  speeds, not only in one  $x$  layer but in all, are entirely independent of  $x$ ; though the *number* of particles in the layer depends on  $x$  alone. Prof. Boltzmann's remark about my quotation from De Morgan will now be seen to be somewhat irrelevant so far as I am concerned, though he may (perhaps justly) apply it to some of his own work.

*Sixth.* As to the Mean Path, though I still hold my own definition to be the correct one, I would for the present merely say that Prof. Boltzmann entirely avoids the statement I made to the effect that those who adopt Maxwell's definition, which is not the ordinary definition of a "mean," must face the question "Why not . . . define the mean path as the product of the average speed into the average time of describing a free path?" The matter is, however, of so little moment, that a very great authority, whom I consulted as to the correct definition of the Mean Free Path, told me that the preferable one was that which lent itself most readily to integration.

*Seventh.* In his remarks upon the effect of external potential, Prof. Boltzmann does not defend his proof to which I objected, but gives a new and fearfully elaborate one. And he quotes, as a remark of mine on this entirely different proof, the phrase "this remarkable procedure" which I had applied to his objectionable old one! He also treats in a disparaging manner the assumption on which my very short investigation is based; viz. "*When a system of colliding particles has reached its final state, we may assume that on the average for every particle which enters, and undergoes collision in, a thin layer, another goes out from the other side of the layer precisely as the first would have done had it escaped collision.*" Of course it would be easy to make a 20 page proof

of this by the help of an imposing array of multiple integrals. But this would be the sort of thing which I have called "playing with symbols," *i. e.* using them *instead of thought*, while their proper function is to *assist thought*. A mathematical demonstration does not *necessarily* imply the use of symbols, any more than that of diagrams:—and, when we find an author continually using symbols to establish what is obvious without them, we very naturally question the validity of his symbolical processes when they are employed for their legitimate purpose. I still think the assumption above a legitimate and indeed almost an obvious one; but it is strange that an objection of this kind should come from a writer like Prof. Boltzmann, who (see head *Second* above) has made, and still defends, a fundamental assumption (of the class to which he applies the term "unbewiesene Voraussetzung") which most clamantly demands proof.

Finally, as Prof. Boltzmann objects alike to Greek, and to English, quotations, although they have Plato and De Morgan for their authors, what does he say to the Latin one

*"Quis tulerit Gracchos de seditione querentes"?*

XXI. *On the Application of the Electrolysis of Copper to the Measurement of Electric Currents.* By THOMAS GRAY, B.Sc., F.R.S.E.\*

IN the Philosophical Magazine for November 1886 I published results of experiments on this subject which had been conducted in the physical laboratory of Glasgow University. Since that time the electrolysis of copper has been extensively employed in that laboratory for standardizing Sir William Thomson's new Standard Electric Balances, and considerable additional experience has thus been obtained as to the accuracy of the method. As the result of that experience, it may be confidently asserted that the constant of an electric-current instrument can be obtained with certainty within a twentieth per cent. of absolute accuracy by copper electrolysis. Although the mode of manipulation that seemed most likely to lead to satisfactory results, and the precautions to be attended to, were pretty fully given in the paper above referred to, Sir William Thomson has called my attention to one or two defects in it which seem to render a supplementary paper desirable.

When discussing the proper value to be taken as the electro-

\* Communicated by the Author.

chemical equivalent of copper, I pointed out that the current-density at the surface of the cathode required to be taken into consideration, because the plates gradually lost weight in the solution. Beyond a general statement of the approximate correction to be allowed for increased area of the cathode plate, and a remark that it depended to some extent on the temperature, little information was given on this somewhat important part of the subject. In order to remove this defect, advantage has been taken of the continual use of electrolysis in the standardizing experiments already referred to; and a large number of electrolytic cells with plates of widely differing areas have generally been used in each experiment, in order to accumulate information on the subject. The effect of temperature has also been more fully investigated. In some of the special experiments on the effect of temperature, two sets of cells at different temperatures were simultaneously used for the purpose of finding whether the deposits were the same for very small plates. The results showed that the same deposit is obtained at all temperatures if the plate be small enough. The smallness of the plate is, however, limited by the necessity of obtaining a good deposit. In the majority of cases one set of plates only was used, the object being to find the relative deposit on plates of different sizes for several temperatures ranging between  $0^{\circ}$  and  $35^{\circ}$  Centigrade.

The solution used for these experiments had a density varying between 1.15 and 1.18, and always contained free acid. The solution was generally made to density 1.18, and one per cent. of sulphuric acid added. This solution was then used for an aggregate time of about ten hours, no additional acid being added, and was then discarded. The volume of solution used per square centimetre of plate-surface immersed was about three cubic centimetres, and the whole of the solution for the set of cells was mixed between each pair of experiments. Ordinary commercial copper sulphate was used in nearly all cases, and it was dissolved in water simply taken from the Glasgow water-supply, which is, however, almost pure. Previous experiments showed that there was no appreciable difference between the results obtained with solutions made in this way and solutions made from pure sulphate dissolved in distilled water. In the method of preparing the plates previous to immersion in the cells, practically no change has been made since the previous paper was written. The plates have generally been simply polished with fine, clean, dry glass-paper, the loose dust wiped off with a clean silk cloth, and the plates then weighed. For polishing the plates a cylinder covered with clean glass-paper is fixed in the lathe



and made to revolve rapidly, while the plate is held lightly against it by means of a pad. This avoids touching either the paper or the part of the plate to be put in the liquid with the fingers. A clean circular brush or a soft silk or velvet pad at the end of the cylinder, and revolving with the paper, is convenient for cleaning off any dust or loose glass from the surface of the plate after it is polished. A set of five or ten plates may be cleaned in this way in a few minutes, and never fail to give a firm deposit and accordant results. The method of washing and drying the plates after they are removed from the cells, which was recommended in my former paper, has been adhered to throughout. As the plates were removed from the cells they were rinsed two or three times in clean water very slightly acidulated, and then placed in a shallow glass tray containing pure water. From this they were almost immediately taken, dried first in a clean pad of blotting-paper, and finally in front of a bright fire, care being taken not to heat the plates much.

The perfect regularity of the results of experiments such as those here referred to is greatly interfered with by the accidental inaccuracies of small amount which are almost unavoidable. The total difference of weight between one plate and the next of a series did not as a rule amount, at ordinary temperatures, to more than a few tenths of a milligramme; and this was subject to error, which might be comparable with its total amount, from inaccuracy in weighing, from want of perfect insulation between the cells, from slight oxidation of the plate in the drying, and so on. The results of the individual experiments were, however, in very good agreement; and I believe the mean results as given in the tables and curves below will be found to be very close to the truth.

An inspection of the tables and curves will show that the effect of temperature, especially when that reaches about 30° Centigrade, is very important. The results become so sensitive to difference of temperature at about 35° C., that a difference of a degree or two between the different cells produces marked irregularity in the curve. At the ordinary temperature of a laboratory in this country, say from 10° C. to 15° C., the effect of difference of temperature is not important, and the numbers given for 12° C. in the table will be found near enough for most purposes. In hot climates, however, where the temperature of the laboratory may be 35° C., or even more, the variations of temperature would require to be very carefully attended to.

Some experiments made for the purpose of testing whether  
*Phil. Mag.* S. 5. Vol. 25. No. 154. March 1888. O

at the high temperatures a correction could be obtained by keeping a plate of the same size as that used in the electrolysis in a similar cell with no current passing through, showed that this method of working was capable of giving approximately correct results. They are hardly ever absolutely exact, but they should not differ from the truth by as much as one tenth per cent. if the current-density is greater than the hundredth of an ampere per square centimetre of cathode surface.

The curve for  $2^{\circ}$  C. is interesting as showing that, within a considerable range of current-density, there is absolutely no effect produced by variation in size of plate at this temperature. Why there should be a somewhat pronounced effect when the size of the plate passes about two hundred square centimetres to the ampere is puzzling, and probably indicates a protecting-power in the passage of the current through the cell. There is some slight evidence of such protection for high current-densities at all temperatures. The curious shape of the curve for the temperature  $12^{\circ}$ , which, as will be seen from Table I., again becomes steeper for lower current-densities than those shown in the curve, is very interesting. This result is certainly genuine, as that curve is taken from the mean of a large number of experiments made under varying conditions by different persons, and the whole of the individual experiments agree closely with the mean curve.

Table I. and the diagram of curves which illustrate it give the relative results for different sizes of cathode, the point of crossing of the curves being reduced to 10,000 for the purpose of rendering the appropriate percentage correction for any particular size of plate easily estimated. With regard to the curves at  $2^{\circ}$ ,  $23^{\circ}$ ,  $28^{\circ}$ , and  $35^{\circ}$  C., it should be stated that the curve for  $2^{\circ}$  is the mean of three experiments, that for  $35^{\circ}$  the mean of two experiments, while those for  $23^{\circ}$  and  $28^{\circ}$  are single experiments.

TABLE I.

Area of cathode, in square centim- etres per am- pere of current.	Tempera- ture, $2^{\circ}$ C.	Tempera- ture, $12^{\circ}$ C.	Tempera- ture, $23^{\circ}$ C.	Tempera- ture, $28^{\circ}$ C.	Tempera- ture, $35^{\circ}$ C.
50 .....	10,000	9996.4	9995	9993.5	9980.5
100 .....	10,000	9987	9983	9979.5	9958
150 .....	9998	9979.5	9974	9969.6	9935
200 .....	9990	9973	9966.6	9957.5	9912
250 .....	9984	9970	9959.5	9939.3	9890
300 .....	9980	9968.5	9952	9920	9868.5
400 .....	.....	9961			
500 .....	.....	9949			

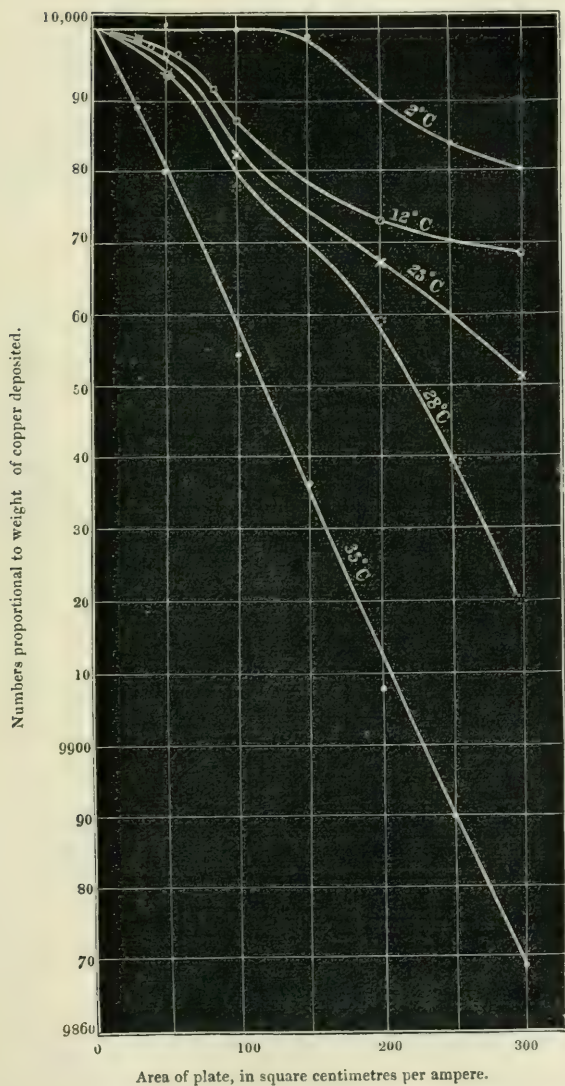


Table II. gives the apparent electrochemical equivalent of copper for the different current-densities and temperatures stated in the table; it is simply the numbers in Table I. multiplied by the proper constant. The results in this table are, as in the former paper, based on the results obtained by Lord Rayleigh and Mrs. Sidgwick for silver, or '001118 grm. of silver deposited per coulomb of electricity passed through the cell. The ratio of the electrochemical equivalents of copper and silver obtained in the experiments described in my former paper has been assumed as correct (Phil. Mag. Nov. 1886). It is '2940 when the copper plate presents a surface of 50 square centimetres per ampere of current.

TABLE II.

Area of cathode, in square centi- metres per am- pere of current.	Tempera- ture, 2° C.	Tempera- ture, 12° C.	Tempera- ture, 23° C.	Tempera- ture, 28° C.	Tempera- ture, 35° C.
50 .....	·0003288	·0003287	·0003286	·0003286	·0003282
100 .....	·0003288	·0003284	·0003283	·0003281	·0003274
150 .....	·0003287	·0003281	·0003280	·0003278	·0003267
200 .....	·0003285	·0003279	·0003277	·0003274	·0003259
250 .....	·0003283	·0003278	·0003275	·0003268	·0003252
300 .....	·0003282	·0003278	·0003272	·0003262	·0003245

**XXII. On a New Method of Reducing Observations relating to Several Quantities.** By F. Y. EDGEWORTH, M.A., Lecturer at King's College, London\*.

A SUBSTITUTE for the *Method of Least Squares* has been proposed by me†, based upon the following principle. The data being of the form

$$a_1x + b_1y \dots - v_1 = 0; \quad a_2x + b_2y \dots - v_2 = 0; \quad \&c.,$$

(where  $v_1, v_2, \&c.$  are observations of equal worth), a solution is obtainable by taking  $x, y \dots$  such that the sum of the residuals (the left-hand members of the above-written equations), *each residual taken positively*, should be a minimum‡.

\* Communicated by the Author.

† Philosophical Magazine, August 1887; and *Hermathena* (Dublin), 1887.

‡ This rule is derivable from the hypothesis that the law of error, the facility-curve under which the observations range, is of the form  $y = \frac{h}{2} e^{-hx}$ ;  $x$  taken positively in both directions. But the use of the rule does not commit us to the assumption of the hypothesis. The Method of Least Sum is in this respect exactly on a par with the Method of Least Squares. The rule of the latter Method is—Determine  $x$  and  $y$  so that the *sum of*



The point thus designated must be on each of two, or more, loci analogous to the Normal equations of the ordinary method. Accordingly the intersection of the "Median loci" was at first proposed by me as the solution. But Mr. Turner has shown that these loci are apt to have in common, not only several points, but even lines and spaces\*. Instructed by his masterly criticism, I now restate the rule as follows; confining myself, for convenience of enunciation, to the case of two unknown quantities.

Trace the observation-lines in the neighbourhood of the true point approximately known†. Beginning at a point on one of those lines, move continually‡ along one or other of the lines in the direction for which  $\frac{dR}{ds}$  is least; R being the sum of the residuals, each taken positively§. The value of  $\frac{dR}{ds}$  for any path  $ax + by - v = 0$  at any assigned point is thus to be ascertained. Let the sum of the  $a$ 's (coefficients of  $x$ ) belonging to the lines on the right of that point be A; and, to

*the squares* of the residuals may be the least possible. This rule is derivable from, and specially correlated with, the hypothesis that the law of facility is the Probability-curve. But it is thought legitimate by Laplace and other eminent authorities to employ the rule even where the hypothesis is not assumed. No doubt the use of either method divorced from the law of facility appropriate to it is open to logical objections. But the difficulties are not greater for one method than for the other. The present writer's explanation of the philosophical difficulty common to both methods is stated in the Appendix to a little treatise on the Art of Measurement, entitled 'Metretike' (London: Temple Co., 1887). It is briefly summarized in the 'Cambridge Philosophical Transactions' for 1887.

\* "On Mr. Edgeworth's Method of Reducing Observations relating to Several Quantities," by H. H. Turner, M.A., B.Sc., Fellow of Trinity College, Cambridge. Philosophical Magazine, December 1887.

† We may, as Mr. Turner says, "leave out of consideration those with large residuals." Or, more exactly, those for which the residual, *divided by the precision*, is large. For the residual  $ax + by - v$  represents the perpendicular let fall from the point  $xy$ , multiplied by the factor  $\sqrt{a^2 + b^2}$ ; which factor may be called the precision. That perpendicular, then, might be short, and the line might run into the little field which we have to explore, if, though the residual is large, it is matched by a large coefficient of precision.

‡ The true point is in general on an observation-line. For, as Mr. Turner has pointed out, the Median loci are in general made up of observation-lines. The exceptions to this statement will be noticed presently.

§ That is, supposing all the  $v$ 's to be observations of equal worth, ranging under one and the same facility-curve. Otherwise it is proper to multiply each of the residuals entering into R by a factor proportional to the Greatest Ordinate of the corresponding facility-curve (supposed symmetrical). See Laplace, *Théor. Analyt.* Suppl. 2, *sub finem*.

the left,  $A'$ . Let the sum of the  $b$ 's, above, be  $B$ ; below,  $B'$ . Then

$$dR = (A' - A)dx + (B' - B)dy;$$

while

$$a dx + b dy = 0, \quad \text{and} \quad ds = \sqrt{dx^2 + dy^2}.$$

When the starting-point is an intersection, we should put ourselves at the *proximate point* along the path which is being explored. To find  $\frac{dR}{ds}$  for another line passing through the same point, or for a neighbouring point and line, it will not be necessary to evaluate  $A A'$ ,  $B B'$  afresh. The coefficients of  $dx$  and  $dy$  in the expression for  $dR$  require alteration only in respect of the coefficients of those lines which have been crossed.

Continue moving according to these directions, until a point is reached at which  $\frac{dR}{ds}$  is positive for every path passing through the point. This point constitutes the solution\*. It is in general unique. For, if possible, let there be two such minimum-points. Take the line joining them as the axis of  $y$ . Then the expression  $(b_1y - r_1) + (b_2y - r_2) + \&c.$ , each of the bracketed terms taken positively, must be a minimum for two points on the ordinate. Which is in general absurd; being possible only in the exceptional case when, in the notation above employed,  $B + b$  is exactly equal to  $B'$ , or  $B' + b$  to  $B$ . In this rare case the solution may be indeterminate, namely any point on a certain line or even area†.

In this event common sense seems to dictate that we should adopt the *middle* of the indeterminate tract as the best point; and this presumption is confirmed by a formal calculation of utility such as Laplace, in the simplest case of a single unknown quantity, has employed to discover the "most advantageous" point‡.

\* The method may be illustrated thus:—Let  $C - R$  (where  $C$  is a constant) represent the height of a surface, which will resemble the roof of an irregularly built slated house. Get on this roof somewhere near the top, and, moving continually upwards along some one of the edges or *arrêtes*, climb up to the top. The highest position will in general consist of a solitary pinnacle. But occasionally there will be, instead of a single point, a horizontal ridge, or even a flat surface.

† This is Mr. Turner's "special case," *loc. cit.* pp. 468 & 469.

‡ Laplace, dealing with a set of observations known to have emanated from a Probability-curve (*Théor. Analyt.* book 2, chap. 4, art. 23), thus in effect reasons:—In the long run of cases, where we have to do with a set of observations exactly the same as the proposed set, the real point, which is the scurce from which this grouping emanated, occurs at different points with a frequency which is represented by a certain Probability-

It may be worth while showing that, not only is the indeterminateness in question rare and remediable, but it is also apt to be slight and neglectible. For it tends to be of an order which is insignificant in comparison with the *probable error* to which the solution is liable. It will be sufficient to show that, in the case of a single quæsitum and observations obeying the simplest law—ranged under one and the same probability-curve—as the number of observations increases, the space which is left blank at the centre tends to become indefinitely small in comparison with the probable error of the solution by the Method of Least Squares. The Modulus of that error is  $\frac{c}{\sqrt{n}}$  (where  $c$  is the Modulus of the primary Probability-curve). Now consider the probability that a certain fraction of that Modulus, say  $i \frac{c}{\sqrt{n}}$ , measured from the centre (the real point supposed known) will have remained blank. The probability that any single observation should fall outside the space  $\pm i \frac{c}{\sqrt{n}}$  may be written  $\left(1 - \theta\left(i \frac{c}{\sqrt{n}}\right)\right)$ , where  $\theta$  is the integral of the error-function. Hence the probability that all the observations should lie outside that limit is

$$\left(1 - \theta\left(i \frac{c}{\sqrt{n}}\right)\right)^n.$$

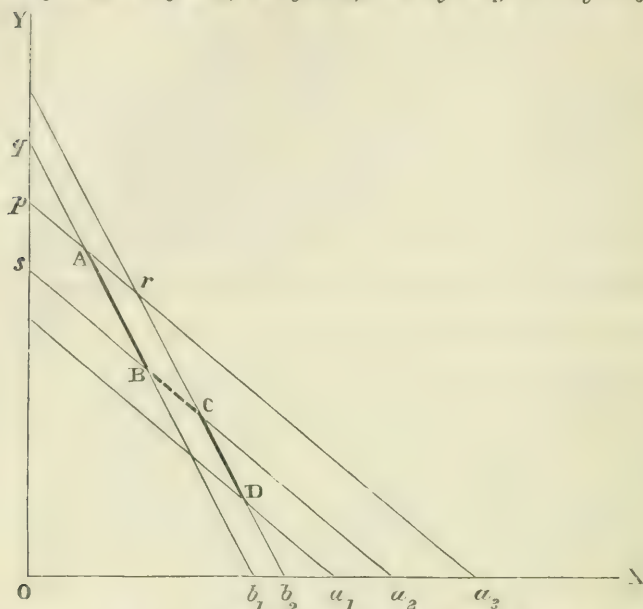
Expanding and taking logarithms, we see that the logarithm of this probability is of the order  $-\sqrt{n}$ ; that is, it becomes indefinitely improbable that the  $i$ th part of the probable error should be left indeterminate. This investigation may be extended to prove the required proposition in its generality.

curve whose centre is the Arithmetic Mean of the given set (and whose Modulus is the  $\frac{1}{\sqrt{n}}$ -th part of the Modulus appertaining to the given observations). If, now, we must put one point as representative of all the series of points which the source may assume, the *best* representative, that which minimises the detriment incident to inevitable error, is the centre of the curve of sources; that is, the Arithmetic Mean of the given observations. Now had the curve of sources given by Inverse Probability consisted of a *horizontal line* at the centre, as in the case before us, the reasoning by which the central point is judged best would not have been affected.

I do not forget that, when the law of error is other than  $y = \frac{h}{2} e^{-hx}$ , this reasoning is applicable less directly, and only in virtue of the explanations referred to in the third note to p. 184.

The rule and the exception may be illustrated by the following example\*. Let the equations be

$$x + y = a_1, \quad x + y = a_2, \quad x + y = a_3, \quad x + \cdot 6y = b_1, \quad x + \cdot 6y = b_3$$



represented by the annexed diagram. Beginning at the intersection A we have, for movement in the direction Ap,

$$dR = dy(2 - 1\cdot 2); \quad ds = \sqrt{2} \, dy;$$

and therefore

$$\frac{dR}{ds} = + \frac{\cdot 8}{\sqrt{2}}.$$

For Aq we have

$$dR = -2dx + dy(3 - \cdot 6),$$

and

$$dx + \cdot 6 \, dy = 0;$$

whence

$$\frac{dR}{ds} = + \frac{3\cdot 6}{\sqrt{1\cdot 36}}.$$

Again, for Ar we have  $\frac{dR}{ds} = 0$ ; and for AB,  $= -\frac{\cdot 4}{\sqrt{2}}$ . AB

\* This example also illustrates the possibility of the intersection between the Median loci being thoroughly indeterminate. The broken line p A B C D a<sub>1</sub> is the locus of Median both for ordinates and coordinates.



is therefore the required path. Proceeding to B we find  $\frac{dR}{ds}$ , for the paths Bs and Bb<sub>1</sub>, *positive*; and for the path BC, *zero*. Also at the point C, all the paths except CB present a positive increment. Hence the *most probable* solution is any point on the line BC; and the *best\** solution is the middle of that line.

Thus the weighty objection to the new method on account of its "failure to give a unique solution" † is completely removed. It remains to consider the objection that it is not less laborious than the ordinary method.

Of course the issue cannot be decided on the score of convenience alone. We must take accuracy also into account. For this purpose we may distinguish two species of cases: (α) those in which the Method of Medians has an advantage in respect of accuracy, and (β) those where the Probability-curve is presumed to prevail. (α) Suppose that the law of facility is "discordant," made up of two Probability-curves, thus:

$$y = \frac{1}{2} \frac{1}{\sqrt{\pi C}} e^{-\frac{x^2}{C^2}} + \frac{1}{2} \frac{1}{\sqrt{\pi c}} e^{-\frac{x^2}{c^2}}.$$

By a formula of Laplace‡, the probable error of the Median is the reciprocal-of-the-Greatest-ordinate divided by  $\sqrt{2n}$ ; that is, in the case before us,

$$\sqrt{\frac{\pi}{2n}} \times \frac{1}{2} \frac{Cc}{C+c}.$$

The corresponding error for the Arithmetic Mean prescribed by the Method of Least Squares is the square root of twice the Mean-Square-of-Error, divided by  $\sqrt{n}$ ; that is,

$$\frac{\sqrt{C^2 + c^2}}{\sqrt{2n}}.$$

It is evident that, if C and c be very unequal, the former solution may be ever so much better than the latter.

(β) In the ordinary case of laws of facility which are Probability-curves, I should like to express myself with a cautious deference to the opinion of practical astronomers. On the one hand, the probable error is increased by about twenty per cent. when we substitute the Median for the Arithmetic or Linear Mean§. On the other hand, the labour of extracting the former is rather less: especially, I should

\* See note 3 to p. 186.

† Supplément 2, *Théor. Anal.*

‡ Mr. H. H. Turner, *loc. cit.*

§ *Ibid.*

think, in the case of many unknown variables. At the same time that labour is more "skilled." There may be needed the attention of a mathematician; and, in the case of many unknowns, some power of hypergeometrical conception. Perhaps the balance of advantage might be affected by an *à priori* knowledge of an approximate solution. Which is, I think, a greater convenience in the case of the new method than of the received one, where the given equations are linear. Again, in cases where *Discordance* or other irregularity may be suspected, it will be useful to verify the result of the Method of Least Squares by the simpler method. The consilience between the two results which Mr. Turner's work exhibits is surely very satisfactory.

Moreover, in cases where the observation-equations are of the second (or higher) degrees, it seems possible that the Median should have a decided advantage over the received method. According to the latter, we must start with an approximation sufficiently close to warrant the neglect of those terms of the equations which involve the variables in the second (or higher) degrees. Otherwise the principle of *Least Squares* would lead at best\* to cumbrous simultaneous equations of the third (or higher) degrees. But, according to the principle herein set forth, a solution is obtained by taking  $x$  and  $y$  such that  $R$ , the sum of the residuals of the type  $[ax^2 + 2hxy + by^2 + 2fx + 2gy - r]$ , each taken positively, should be the least possible. By considering the geometrical interpretation of this condition, we may see that the required point is on an observation-curve. As in the simpler case, we may climb to the position of highest probability by noting the value of  $\frac{dR}{ds}$  at every turn. Thus, let us start from a point  $x_0, y_0$

on the curve  $a_0x^2 + 2h_0xy + b_0y^2 + 2f_0x + 2g_0y = r_0$ : and, as before, let the coefficients of the observation-lines *below* and *left* of the initial point be dotted; *above* and *right*, plain. Then

$$dR = 2dx[(Sa' - Sa)x_0 + (Sh' - Sh)y_0 + (Sf' - Sf)] \\ + 2dy[(Sb' - Sb)y_0 + (Sh' - Sh)x_0 + (Sg' - Sg)];$$

where

$$dx[a_0x + h_0y + f_0] + dy[b_0y + h_0x + g_0] = 0.$$

From these equations  $\frac{dR}{ds}$  is to be found. If it is *positive*, we move backwards, and *vice versa*. At each intersection we take the path for which  $\frac{dR}{ds}$  is least.

\* Even supposing the law of facility to be the Probability-curve.

Other uses and qualifications of the method would doubtless come to light, if experts would turn their attention to the subject. It may be hoped that Mr. Turner's example will be followed by other practical astronomers. An unquestioning acquiescence in the orthodox Method of Least Squares is the less justifiable, in so far as the doctrine, in its form of rigid exclusiveness, is not derivable from the fountain of authority, Laplace. Laplace by no means degrades the "*Method of Situation*" below the *Method of Least Squares*. He expressly prefers the former in certain cases\*. Orthodoxy should not put tradition before inspiration.

XXIII. *An Experimental Study on the Influence of Magnetism and Temperature on the Electrical Resistance of Bismuth and its Alloys with Lead and Tin.* By EDMOND VON AUBEL, of Liège, Member of the Physical Society of London†.

#### PRELIMINARY COMMUNICATION.

##### BIBLIOGRAPHY.

1. H. Tomlinson. Proc. Roy. Soc. London, xxxiii. p. 276 (1882).
2. Righi. *Atti della R. Accademia dei Lincei* [3], xix. p. 545 (1883-84); *Journal de Physique* [2], iii. p. 355 (1884).
3. Hurion. *Journal de Physique* [2], iv. p. 171 (1885).
4. Leduc. *Journal de Physique* [2], iii. p. 362 (1884), and v. p. 116 (1886).
5. Von Ettingshausen and Nernst. *Wien. Ber.* xciv. Abth. ii. p. 560 (1886).
6. J. Faé. *Lumière électrique*, xxiii. p. 169 (1887), and xxv. p. 630 (1887).
7. Goldhammer. *Annalen der Physik*, 1887, no. 7, p. 360.
8. Von Ettingshausen. *Sitzungsberichte der Akademie der Wissenschaften zu Wien*, Meeting of the 31st March, 1887.
9. Elie. *Journal de Physique*, September 1887.

MANY physicists have been at work upon the influence of magnetism on the electrical conductivity of metals. The metals which have already been studied are iron, nickel, cobalt, antimony, tellurium, and especially bismuth.

The above investigations have been carried on principally with the view of discovering an explanation of Hall's phenomenon.

The present memoir is intended to fix the date of publication. Our researches were commenced last May; since then many works have appeared on the question which we

\* *Théor. Analytique*, Supplément 2, *sub finem*; referring to the simple case of a single unknown quantity. To generalize the principle there laid down is the object of this paper.

† Communicated by the Physical Society: read January 28, 1888.

were studying, notably the excellent memoirs of Messrs. Goldhammer\*, A. von Ettingshausen†, and W. Nernst‡.

Quite recently we saw, in the *Wiener Anzeiger*, 1887, p. 222, that Messrs. von Ettingshausen and Nernst had made a communication to the Academy of Sciences at Vienna, having the title "Ueber das thermische und galvanische Verhalten einiger Wismuth-Zinn-Legirungen im magnetischen Felde." This paper is not yet published, as far as we know; so that we cannot take account of it in what follows.

Our researches are far from being finished. They will be continued as soon as our other occupations will permit.

### I. The Preparation of Rods of Bismuth.

We have studied bismuth under three molecular states :—

- (1) Melted and cooled slowly,
- (2) Melted and rapidly cooled or tempered,
- (3) Compressed.

Let us examine the preparation of rods of bismuth in these three cases.

1. In order to prepare bismuth wires slowly cooled, capillary glass tubes were procured, to which were joined at both ends, at right angles to the capillary part, glass tubes of fairly large diameter. These capillary tubes were heated in a sand-bath, and some grains of bismuth were introduced into one of the side branches. When the metal was melted, it ran down by its own weight into the capillary tube. The capillary tubes were then left to cool very slowly; they were taken from the sand-bath only when it was quite cold. It often happens that, owing to the unequal expansions produced during the cooling of the bismuth and the glass, the tubes are broken; for this reason we used capillary tubes with very thin walls.

Working in this way, any formation of oxide in the capillary part is entirely avoided; and this part alone must be used in the experiments.

We think that this way of preparing bismuth wires is much more convenient than the method by exhaustion employed by others§, and which cannot furnish results entirely comparable,

\* We have given in the Bibliography only the list of works relating to bismuth.

† *Vide* the Bibliography.

‡ *Annalen der Physik*, 1887, No. 8b.

§ See notably the memoir of Leduc, *Journal de Physique* [2], iii. p. 363.



because there is no certainty that the melted metal is always cooled under the same conditions. The bismuth rod being thus prepared, the two side branches which are not wanted are taken away.

The electrodes to convey the electric current remain to be fixed. For this purpose one end of the capillary rod of bismuth is heated at the same time as a platinum wire in a Bunsen burner up to the melting-point of bismuth; then the red-hot platinum wire is plunged into the capillary tube. It is easy to see that a good contact is thus produced.

2. In order to obtain bismuth rods very rapidly cooled or tempered, the melted metal was poured rapidly into a kind of iron trough, cold, and forming a somewhat acute dihedral angle. If the pouring takes place rapidly, by inclining the trough sufficiently to cause the melted bismuth to run down very quickly, rods of bismuth can be obtained sufficiently long and not too thick. This last condition is indispensable, otherwise the bismuth rods will have too small a resistance.

No adherence takes place during this tempering between the iron and the bismuth. Moreover, by taking certain precautions, the surface of the bismuth may also be filed, and a fresh part of the metal be thus exposed.

In this case the electrodes were made of copper wire, soldered to the two ends of the rod of bismuth by means of a fusible alloy.

3. The compressed bismuth which we used in our experiments was given to us by Prof. Spring. Having been prepared in a drawplate\*, it was absolutely cylindrical.

The electrodes were formed, as in the case of the tempered bismuth, of copper wires soldered to the bismuth with a fusible alloy.

The molecular arrangement in this compressed bismuth is very remarkable. A fracture normal to the axis of the cylinder presents a radial structure. We shall see later on the curious results that this sample has given us.

The alloys have only been studied under two states :—

- (1) Melted and very slowly cooled,
- (2) Tempered.

Rods of these alloys have been prepared in the same way as those of bismuth.

\* *Annales de la Société Géologique de Belgique*, t. xi. p. cxxxiv (1884).

## II. *Alloys.*

To obtain the alloys, two sandstone crucibles are used. Bismuth is put into one, tin or lead into the other. These metals have previously been weighed. When the bismuth is melted it is kept liquid, being warmed gently to avoid too much oxidation; it is then poured upon the other metal, which is also liquid. The contents of the crucible are then poured into the other, and this decanting is repeated several times. We have worked in this way in order to obtain alloys in which the proportion of tin or lead should be given almost exactly by the weight, and to avoid having to make analyses of the alloys in order to know their approximate composition.

## III. *Analyses of the different Specimens of Bismuth studied.*

We have measured the resistance of several specimens of bismuth. They came from the manufactory of chemical products of Messrs. Monheim at Aix-la-Chapelle, Trommsdorff at Erfurt, and Schering at Berlin. Mr. Monheim's bismuth is commercially pure metal. We have three different specimens from the firm of Trommsdorff, which we shall call, for brevity, "Trommsdorff I.," "Trommsdorff II.," and "absolutely pure Trommsdorff."

The two first, as well as the bismuth from the Schering factory, are products referred to as very pure in the catalogue of manufacturers.

The "absolutely pure Trommsdorff" bismuth, of which the cost was far higher than that of the others, has been specially prepared for us in the Erfurt manufactory.

As to the compressed bismuth, it has been obtained by means of the commercial metal. Prof. Spring dissolves this metal in  $\text{HNO}_3$ , precipitates it by pouring the solution into a large quantity of water, filters, and washes it. He then redissolves the basic nitrate, precipitates a second time, calcines, and reduces in a current of pure hydrogen.

We give all these directions, so that it may be possible for physicists to repeat our experiments, and in order to show how little confidence ought to be placed in certain determinations which have been made on the physical constants of bismuth.

M. A. Classen, Professor of Analytical Chemistry at the Upper Technical School at Aix-la-Chapelle, was so kind as to

undertake to make qualitative analyses of various specimens for us. We wish again to thank him here for the indispensable assistance which he has given to our work.

Results of the qualitative analyses :—

Bismuth.	Impurities.
Monheim . . .	<i>Copper*</i> , <i>lead</i> , iron.
Trommsdorff I. .	<i>Iron</i> , copper.
Trommsdorff II. .	<i>Lead</i> , copper, nickel.
Absolutely pure Trommsdorff . }	<i>Lead</i> , copper (traces), <i>iron</i> , carbon.
Schering . . .	Copper, lead ?, nickel.

It is very difficult to prepare pure bismuth. Even the method indicated above for preparing the metal which has been compressed, does not yield a pure product. A small quantity of lead and copper is always precipitated with the bismuth ; and it is only by repeating this operation several times that the metal can be obtained very pure.

M. A. Classen is occupied at the present time in preparing for us pure bismuth, which is to be used in our later researches.

It results from all this that the various specimens of so-called pure bismuth are very different from a chemical point of view. Their points of fusion and their specific weights have been likewise determined ; they vary very sensibly from one sample to another.

#### IV. *Measurement of Electrical Resistances.*

The threads of bismuth were placed in a water-bath, which was heated all along its length by a linear gas-burner, forming a series of small flames : a thermometer indicated the temperature.

The whole was placed between the poles of a Ruhmkorff's electromagnet of large size, worked by an Otto gas-engine and a dynamo-electric machine of Siemens and Halske†.

The intensity of the electric current in the spirals of the electromagnet was sensibly 28 amperes during the whole course of the experiments.

\* The metals whose names are in italic are those which were found in considerable quantities in the bismuth analysed.

† The electromagnet was set in such a direction as to have no influence on the galvanometer.

The ordinary poles of the electromagnet were replaced by large flat poles, made of thick iron circular plates, each one having a thickness of 15 millimetres and a diameter of 150 millimetres. In this way the magnetic field was made more uniform.

In order to measure the resistances, Thomson's method was employed, and a Siemens's *dead-beat* galvanometer, with the magnet in the form of a bell, was used.

In order to avoid heating the thread of bismuth by the passage of the current, one Grove cell only was used, and, by the aid of a see-saw commutator, the current was not allowed to pass in the bismuth for a longer time than was necessary for the observations.

The deviations of the galvanometer-mirror were observed in a telescope furnished with a graduated scale, the distance between each division being two millimetres, and placed at a distance of more than seven metres from the mirror. One can judge of the sensitiveness of the method. We have also employed Kirchhoff's method of measuring resistances, preserving all the other arrangements. A great number of rods of bismuth and its alloys have been tried; in the following tables we only give the values obtained for a sample of each kind.

In our experiments we did not obtain directly the resistance of the thread of bismuth, because in Thomson's method the wires for conveying the current were not fixed directly to this metal, but to the copper or platinum conductors which served as electrodes. It was necessary therefore to deduct the resistances of these copper or platinum conductors, in order to obtain values relating only to the bismuth. In the case of the wires slowly cooled, we have subtracted the values obtained directly, as the resistance of the platinum conductors exterior to the wire of bismuth, because this latter metal adheres very easily to platinum, and the section of the rod of bismuth is very great when compared with that of the thread of platinum.

### V. Results of Electrical Measurements.

The values of  $W$  are the electrical resistances in Siemens's units; the values of  $W_m$  are the electrical resistances in the same units under the action of a magnet.



(a) <i>Rods of Bismuth slowly cooled.</i>			
	Temp.	W.	W <sub>m</sub> .
Trommsdorff bismuth, absolutely pure.....	17·2	0·4100	0·4120
	42·7	0·4050	0·4070
	70·8	0·3953	0·3966
Trommsdorff bismuth I. {	20·4	0·1000	0·1010
	42·4	0·1092	0·1099
	69·9	0·1210	0·1217
Monheim bismuth ..... {	18·6	1·1295	1·1455
	41·2	1·0425	1·0525
	72·2	0·9375	0·9435
Schering bismuth..... {	18·2	0·2522	0·2572
	43·5	0·2652	0·2662
	71	0·2789	0·2789
Trommsdorff bismuth II. {	20·4	0·1661	0·1671
	43	0·1713	
	73	0·1811	0·1821

(b) <i>Rods of Bismuth tempered.</i>			
Trommsdorff bismuth, absolutely pure.....	15·2	0·1788	0·1790
	40·7	0·1805	
	69·7	0·1812	0·1812
Monheim bismuth ..... {	18·4	0·0934	0·0940
	43	0·0932	0·0937
	70	0·0909	0·0911
Trommsdorff bismuth II. {	16·2	0·0784	0·0789
	40·3	0·0774	0·0777
	74·2	0·0765	0·0770
Schering bismuth..... {	20·2	0·1239	0·1254
	42	0·1239	0·1249
	72·4	0·1244	0·1249
Trommsdorff bismuth I. {	18·2	0·0510	0·0515
	41·6	0·0512	0·0517
	71·4	0·0541	0·0545

(c) <i>Alloys with Tin.</i>			
(1) <i>Rods slowly cooled.</i>			
Absolutely pure Tromms- dorff bismuth. {	19	0·3364	0·3374
	42·8	0·3444	0·3454
	71·6	0·3475	0·3475
0·39 gr. tin to 100 gr. Bi. {	19·6	0·3966	0·3976
	45·2	0·4066	0·4066
	70·6	0·4166	0·4166
Trommsdorff bismuth I. {	19·8	0·3643	0·3663
	41·2	0·3733	0·3743
	69·8	0·3803	0·3813
Schering bismuth. {	19·8	0·3643	0·3663
	41·2	0·3733	0·3743
	69·8	0·3803	0·3813
0·40 gr. tin to 100 gr. Bi. {	19·8	0·3643	0·3663
	41·2	0·3733	0·3743
	69·8	0·3803	0·3813
Trommsdorff bismuth II. {	21	0·5133	0·5183
	42·6	0·5258	0·5288
	71·8	0·5343	0·5363
0·46 gr. tin to 100 gr. Bi. {	21	0·5133	0·5183
	42·6	0·5258	0·5288
	71·8	0·5343	0·5363

Table (continued).

(c) <i>Alloys with Tin</i> (cont.).			
(2) Rods tempered.			
	Temp.	W.	W m.
Trommsdorff bismuth, {	18.6	0.2133	0.2136
absolutely pure. {	41.6	0.2173	0.2178
0.39 gr. tin to 100 gr. Bi. {	70.4	0.2213	0.2213
Schering bismuth. {	16.6	0.2100	0.2105
0.40 gr. tin to 100 gr. Bi. {	41.8	0.2145	0.2150
	73.9	0.2148	0.2150
Trommsdorff bismuth I. {	20.4	0.1443	0.1443
0.46 gr. tin to 100 gr. Bi. {	42.8	0.1478	0.1478
	69.6	0.1498	0.1498
Trommsdorff bismuth II. {	24.5	0.1529	0.1534
0.46 gr. tin to 100 gr. Bi. {	40.6	0.1554	0.1561
	77.8	0.1574	0.1574

(d) <i>Alloys with Lead</i> .			
(1) Rods slowly cooled.			
Trommsdorff bismuth, {	18.8	0.4925	0.4945
absolutely pure. {	44.7	0.4995	0.5015
0.52 gr. Pb to 100 gr. Bi. {	68.9	0.5015	0.5035
Trommsdorff bismuth I. {	21	0.2713	0.2743
0.54 gr. Pb to 100 gr. Bi. {	43.4	0.2778	0.2788
	71.6	0.2878	0.2888
Schering bismuth. {	18.8	0.2631	0.2656
0.49 gr. Pb to 100 gr. Bi. {	44.8	0.2611	0.2632
	67.4	0.2620	0.2630
Trommsdorff bismuth II. {	15.4	0.3923	0.3963
0.60 gr. Pb to 100 gr. Bi. {	43	0.3878	0.3913
	69.8	0.3848	0.3871

(2) Rods tempered.			
Trommsdorff bismuth, {	18.2	0.1690	0.1700
absolutely pure. {	42.4	0.1725	0.1730
0.52 gr. Pb to 100 gr. Bi. {	70.4	0.1735	0.1735
Trommsdorff bismuth I. {	17.4	0.0937	0.0947
0.54 gr. Pb to 100 gr. Bi. {	41.6	0.0929	0.0934
	71	0.0914	0.0919
Schering bismuth. {	18.8	0.1478	0.1493
0.49 gr. Pb to 100 gr. Bi. {	44.4	0.1458	0.1468
	70.2	0.1439	0.1449
Trommsdorff bismuth II. {	22.2	0.1056	0.1066
0.60 gr. Pb to 100 gr. Bi. {	43.6	0.1056	0.1061
	72.2	0.1031	0.1031

Table (continued).

(e) <i>Compressed Bismuth.</i> (From Prof. Spring.)			
	Temp.	W.	W <sub>m</sub> .
Compressed bismuth ... {	16.8	0.1148	0.1157
	42.4	0.1140	0.1145
	76	0.1150	0.1153
The same compressed bismuth, melted, cooled very slowly in a sand-bath, and then operated upon again. {	16.2	0.1861	0.1871
	42.3	0.2051	0.2053
	71.7	0.2263	0.2263

VI. *Inferences drawn from the preceding Tables.*

1. *The Influence of Temperature.*—If the preceding results be examined, it is easy to see at once that the different samples of bismuth examined behave quite differently. Some of them give an increased resistance, and others, which is remarkable, a diminished resistance, when the temperature rises. This last fact, noticed for the first time by M. Righi, is not due, according to this physicist, to the presence of arsenic as an impurity in the bismuth, but ought to be attributed to the tin, which, even in very small quantities, is able to produce this result.

A number of experiments have been made by us to discover the reason of this diminution of resistance. We studied first alloys of bismuth and tin, in which the latter metal was present in comparatively large quantities. On examining Table (c), it will be seen that these alloys always give an increase of resistance with the temperature, while the bismuth which enters into their composition produces a diminution of resistance for a rise of temperature. Besides, the chemical analysis has shown us that not one of our samples of bismuth contains any tin as an impurity. It is then sufficiently demonstrated that it is not the metal that is the cause of the observed anomaly\*. A glance at the results of the chemical analyses and at the electrical measurements will equally prove that the reason for the fact ought not to be looked for in the presence, as impurities, either of arsenic or of iron.

Mr. C. W. Kayser† has shown that expansion on solidifi-

\* See also the memoir of Mr. W. Nernst (*Annalen der Physik*, 1887, p. 783), which has appeared during the course of our researches.

† *Berichte der deutsch. chem. Gesellsch. Berlin*, 1869, p. 309.

cation has not taken place when bismuth is combined with 10 per cent. of lead.

For this reason we have examined also the alloys of bismuth and lead, which have furnished us with very interesting results\*, recorded in Table (d), but which do not in any way explain the diminution of resistance when the temperature rises.

Some influence might still be attributed to the capillary glass tube in which the wire of bismuth is placed. On heating, the glass and bismuth expand differently, and mutually impede each other in their movements. We have therefore successively studied a rod of bismuth cast in a capillary tube, and the same rod after having broken the glass (with precautions) in such a way as not to alter the metallic wire; no change was observed in the direction of these phenomena. Besides, the rods of tempered bismuth were not prepared in glass tubes, and yet some of these present the same peculiarity.

We likewise assured ourselves that there was no relation between the abnormal electrical phenomenon and the points of fusion or specific gravities of the different kinds of bismuth†.

2. *Influence of Magnetism.*—On the whole the influence of magnetism on the resistance of bismuth has been more feeble than that which has been announced by M. Righi‡. Magnetism always produces an increase of resistance; its influence diminishes when the temperature rises, and is more feeble in the alloys than in the bismuth itself.

3. *Study of Compressed Bismuth.*—The resistance of compressed bismuth hardly varies with the temperature; from  $16^{\circ}8$  to  $42^{\circ}4$  there was a slight diminution; then an increase up to  $76^{\circ}$ , also very small.

After these first experiments, we took away the two ends of the threads of compressed bismuth, on which was the fusible alloy. With the rest we prepared a rod of bismuth slowly cooled by the method indicated above.

Measurements of the electrical resistances of this bismuth, in this state, gave a fairly large increase of resistance when the temperature rose.

\* M. Leduc had already studied the conductivity of the alloys of lead and bismuth (*Journal de Physique* [2], v. p. 116), but from another point of view.

† Mr. C. L. Weber has made known, in 1886 (*Annalen der Physik* xxvii. p. 145), some very curious results on the influence of temperature on the electrical conductivity of the alloys of Rose, Wood, and Lipowitz; but this work has no immediate connexion with our study.

‡ Mr. Goldhammer (*Z. c.*) has also found the influence of magnetism more feeble than that indicated by M. Righi.



There is, then, a notable difference, with regard to the molecular structure, between bismuth prepared in a draw-plate and that which, having been melted, has been allowed to cool slowly. One may conceive in this latter case that the crystals take up their own positions during the cooling, whilst the metal prepared in a drawplate is formed of parallel fibres of bismuth.

More complete electrical researches might make clear this important question of molecular physics. One might especially compare bismuth, compressed in an ordinary compressor, with that prepared in a drawplate.

### VII. Conclusions.

To sum up, the diminution of electrical resistance observed in certain kinds of bismuth and in some alloys with lead cannot as yet be explained. The molecular structure, which we have modified by tempering and compression, has a great influence on the action of the temperature.

We hope that our further researches will lead us to an explanation of the anomalies which we have pointed out.

In the course of this paper we have never mentioned the name of Prof. Ad. Wüllner, our learned and venerated master; we might well have done so at each step. We are glad to be able here to express our thanks to him for the facilities which he has procured for us for undertaking our researches.

Laboratoire de Physique de l'Ecole  
Technique Supérieure d'Aix-la-Chapelle.  
November 1887.

### NOTE.

M. G. P. Grimaldi has recently \* studied the influence of magnetism on the thermoelectric properties of bismuth. He denotes by  $\epsilon$  the thermoelectric electromotive force of the bismuth-copper couple, when it is out of the magnetic field; by  $\epsilon'$  the electromotive force of the same couple in the magnetic field; and takes for a measure of the phenomenon the quantity  $\delta = \frac{\epsilon - \epsilon'}{\epsilon}$ .

This physicist finds that  $\delta$  is positive for commercial bismuth and negative for pure bismuth.

One ought, perhaps, to compare this result with those we have obtained relative to the influence of temperature on the electrical resistance of different kinds of bismuth.—EDM. VON AUBEL.

\* *Journal de Physique*, Dec. 1887, p. 569. This paper is a résumé written by the author of a preliminary note presented to the R. Accademia dei Lincei (Feb. 7, 1886) and of a memoir presented in June 1886 to the Società di Scienza naturali ed economiche di Palermo.

See also *Beiblätter zu den Annalen der Physik*, 1887, no. 6, p. 472.

XXIV. *Note on a Paper on Electromagnetic Waves.**By* OLIVER HEAVISIDE\*.

**A**N editorial query, the purport of which I did not at first understand, has directed my attention to Prof. J. J. Thomson's paper "On Electrical Oscillations in Cylindrical Conductors" (Proc. Math. Soc. vol. xvii. Nos. 272, 273), a copy of which the author has been so good as to send me. His results, for example, that an iron wire of  $\frac{1}{8}$  centim. radius, of inductivity 500, carries a wave of frequency 100 per second about 100,000 miles before attenuating it from 1 to  $\epsilon^{-1}$ , and similar results, summed up in his conclusion that the carrying-power of an iron wire cable is very much greater than that of a copper one of similar dimensions, are so surprisingly different from my own, deduced from my developed sinusoidal solutions, in the accuracy of which I have perfect confidence (having had occasion last winter to make numerous practical applications of them in connexion with a paper which was to have been read at the S. T. E. and E.), that I felt sure there must be some serious error of a fundamental nature running through his investigations. On examination I find this is the case, being the use of an erroneous boundary condition in the beginning, which wholly vitiates the subsequent results. It is equivalent to assuming that the tangential component of the flux magnetic induction is continuous at the surface of separation of the wire and dielectric, where the inductivity changes value, from a large value to unity, when the wire is of iron. The true conditions are continuity of tangential force and of normal flux.

As regards my own results, and how increasing the inductance is favourable, the matter really lies almost in a nutshell; thus. In order to reduce the full expression of Maxwell's connexions to a practical working form I make two assumptions. First, that the longitudinal component of current (parallel to the wires) in the dielectric is negligible, in comparison with the total current in the conductors, which makes  $C$  one of the variables,  $C$  being the current in either conductor; and next, what is equivalent to supposing that the wave-length of disturbances transmitted along the wires is a large multiple of their distance apart. The result is that the equations connecting  $V$  and  $C$  become

\* Communicated by the Author. This Note may be regarded as a continuation of Note B to "Electromagnetic Waves," Phil. Mag. February 1888.

$$-\frac{dV}{dz} = R''C, \quad -\frac{dC}{dz} = KV + S \frac{dV}{dt};$$

$S$  being the permittance and  $K$  the conductance of the dielectric per unit length of circuit, whilst  $R''$  is a "resistance-operator," depending upon the conductors, and their mutual position, which, in the sinusoidal state of variation, reduces to

$$R'' = R' + L' \frac{d}{dt},$$

where  $R'$  and  $L'$  are the effective resistance and inductance of the circuit respectively, per unit length, to be calculated entirely upon electromagnetic principles. It follows that the fully developed sinusoidal solution is of precisely the same form as if the resistance and inductance were constants. Disregarding the effect of reflexions, we have

$$V = V_0 e^{-Pz} \sin(nt - Qz),$$

due to  $V_0 \sin nt$  impressed at  $z=0$ ; where  $P$  and  $Q$  are functions of  $R'$ ,  $L'$ ,  $S$ ,  $K$ , and  $n$ .

Now if  $R'/L'n$  is large, and leakage is negligible (a well-insulated slowly worked submarine cable, and other cases), we have

$$P = Q = \left(\frac{1}{2}RSn\right)^{\frac{1}{2}},$$

as in the electrostatic theory of Sir W. Thomson. There is at once great attenuation in transit, and also great distortion of arbitrary waves, owing to  $P$  and  $Q$  varying with  $n$ .

But in telephony,  $n$  being large,  $P$  and  $Q$  may have widely different values, because  $R'/L'n$  may be quite small, even a fraction. In such case we have no resemblance to the former results. If  $R'/L'n$  is small,  $P$  and  $Q$  approximate to

$$P = \frac{R'}{2L'v'} + \frac{K}{2Sv'}, \quad Q = \frac{n}{v'},$$

where  $v' = (I/S)^{-\frac{1}{2}}$ . This also requires  $K/Sn$  to be small. But it is always very small in telephony.

Now take the case of copper wires of low resistance.  $L'$  is practically  $L_0$ , the inductance of the dielectric, and  $v'$  is practically  $v$ , the speed of undissipated waves, or of all elementary disturbances, through the dielectric, whilst  $R'$  may be taken to be  $R$ , the steady resistance, except in extreme cases. Hence, with perfect insulation,

$$P = \frac{R}{2L_0v}, \quad Q = \frac{n}{v},$$

or the speed of the waves is  $v$ , and the attenuating coefficient  $P$  is practically independent of the frequency, and is made smaller by reducing the resistance and *increasing* the inductance, *of the dielectric*.

The corresponding current is

$$C = V/L_0v$$

very nearly, or  $V$  and  $C$  are nearly in the same phase, like undissipated plane waves. There is very little distortion in transit.

How to increase  $L_0$  is to separate the conductors, if twin wires, or raise the wire higher from the ground, if a single wire with earth-return. It is not, however, to be concluded that  $L_0$  could be increased indefinitely with advantage. If  $l$  is the length of the circuit,

$$Rl = 2L_0v$$

shows the value of  $L_0$  which makes the received current greatest. It is then far greater than is practically wanted, so that the difficulty of increasing  $L_0$  sufficiently is counter-balanced by the non-necessity. The best value of  $L_0$  is, in the case of a long line, out of reach; so that we may say, generally, that increasing the inductance is always of advantage to reduce the attenuation and the distortion.

Now if we introduce leakage, such that  $R/L_0 = K/S$ , we entirely remove the distortion, not merely when  $R/L_0n$  is small but of any sort of waves. It is, however, at the expense of increased attenuation. The condition of greatest received current,  $L_0$  being variable, is now

$$Rl = L_0v.$$

We have thus two ways of securing good transmission of electromagnetic waves: one very perfect, for any kind of signals; the other less perfect, and limited to the case of  $R/L_0n$  small, but quite practical. The next step is to secure that the receiving-instrument shall not introduce further distortion by the quasi-resonance that occurs. In the truly non-distortional circuit this can be done by making the resistance of the receiver to be  $L_0v$  (whatever the length of the line); this causes complete absorption of the arriving waves. In the other case, of  $R/L_0n$  small, with good insulation, we require the resistance of the receiver to be  $L_0v$  to secure this result approximately. I have also found that this value of the receiver's resistance is exactly the one that (when size of wire in receiver is variable) makes the magnetic force, and therefore the strength of signal, a maximum. Some correction is required on account of the self-induction of the receiver;



but in really good telephones of the best kind, with very small time-constants, it is not great. We see therefore that telephony, so far as the electrical part of the matter is concerned, can be made as nearly perfect as possible on lines of thousands of miles in length. But the distortion that is left, due to imperfect translation of sound-waves into electromagnetic waves at the sending-end, and the reproduction of sound-waves at the receiving-end, is still very great; though, practically, any fairly good telephonic speech is a sufficiently good imitation of the human voice.

There is one other way of increasing the inductance which I have described, viz. in the case of covered wires to use a dielectric impregnated with iron dust. I have proved experimentally that  $L_0$  can be multiplied several times in this way without any increase of resistance; and the figures I have given above (in Note B) prove what a wonderful difference the self-induction makes, even in a cable, if the frequency is great. Hence, if this method could be made practical, it would greatly increase the distance of telephony through cables.

Now, passing to iron wires, the case is entirely different, on account of the great increase in resistance that the substitution of iron for copper of the same size causes, which increases  $P$  and the attenuation. Taking for simplicity the very extreme case of such an excessive frequency as to make the formula

$$R' = (\frac{1}{2} R \mu n)^{\frac{1}{2}}$$

nearly true,  $R$  being the steady and  $R'$  the actual resistance, we see that increasing either  $R$  or  $\mu$  increases  $R'$  and therefore  $P$ , because  $L'v'$  tends to the value  $L_0v$ . Thus the carrying-power of iron is not greatly above, but greatly below that of copper of the same size.

I have, however, pointed out a possible way of utilizing iron (other than that above mentioned), viz. to cover a bundle of fine iron wires with a copper sheath. The sheath is to secure plenty of conductance; the division of the iron to facilitate the penetration of current, and so lower the resistance still more, to the greatest extent, whilst at the same time increasing the inductance. But the theory is difficult, and it is doubtful whether this method is even theoretically legitimate. First class results were obtained by Van Rysselberghe on a 1000-mile circuit in America (2000 miles of wire), using copper-covered steel wire. Here the resistance was very low, on account of the copper, and the inductance considerable, on account of the dielectric alone; so that there is no certain

evidence that the iron did any good except by lowering the resistance. But about the advantage of increasing the inductance of the dielectric there can, I think, be no question. It imparts momentum to the waves and carries them on.

In Note B to the first portion of my paper "On Electromagnetic Waves" (Phil. Mag. Feb. 1888), I gave four sets of numerical results showing the influence of increasing the inductance, selecting a cable of large permittance (constant) in order to render the illustrations more forcible. I take this opportunity of stating that the second set of figures relates to the value  $L = 2\frac{1}{2}$ , not 2, of the inductance per centim. The formula used was equation (82), Part II. of my paper "On the Self-Induction of Wires" (Phil. Mag. Sept. 1886, p. 284), which is

$$C_0 = 2V_0 \frac{(Sn)^{\frac{1}{2}}}{(R'^2 + L'^2 n^2)^{\frac{1}{4}}} (\epsilon^{2Pl} + \epsilon^{-2Pl} - 2 \cos 2Ql)^{-\frac{1}{2}};$$

where

$$P \text{ or } Q = (\tfrac{1}{2}Sn)^{\frac{1}{2}} \{ (R'^2 + L'^2 n^2)^{\frac{1}{2}} \mp L'n \}^{\frac{1}{2}};$$

where  $C_0$  is the amplitude of current at  $z=l$  due to impressed force  $V_0 \sin nt$  at  $z=0$ , with terminal short-circuits. When the circuit is long enough to make  $\epsilon^{-Pl}$  small, we obtain

$$\rho = \frac{(R'^2 + L'^2 n^2)^{\frac{1}{4}}}{2Rl(Sn)^{\frac{1}{2}}} \epsilon^{Pl}$$

as the expression for the ratio  $\rho$  of the steady current to the amplitude of the sinusoidal current.

The following table is constructed to show the fluctuating manner of variation of the amplitude with the frequency. Drop the accents, and let  $R/Ln$  be small. Then, approximately,

$$\rho = \frac{1}{2y} (\epsilon^y + \epsilon^{-y} - 2 \cos 2 \frac{nl}{v})^{\frac{1}{2}},$$

where

$$y = Rl/Lv,$$

under no restriction as regards the length of the circuit. Now give  $y$  a succession of values, and calculate  $\rho$  with the cosine taken as  $-1, 0$ , and  $+1$ . Call the results the maximum, mean, and minimum values of  $\rho$ .

$y$ .	Min. $\rho$ .	Mean $\rho$ .	Max. $\rho$ .	$y$ .	$\rho$ .	$y$ .	$\rho$ .
$\frac{1}{2}$	505	1500	2063	6	1.678	12	16.81
1	521	878	1128	7	2.365	14	39.3
2	587	686	771	8	3.378	16	93.2
2.065	594	685	766	9	5.000	18	225
3	710	718	784	10	7.420	20	550
4	907	924	940				
5	1210	1218	1226				

It will be seen that when the resistance of the circuit is only a small multiple of, or is of about the same magnitude as  $Lv$  (which may be from 300 to 600 ohms in the case of a suspended copper wire), the variation in the value of  $\rho$  as the frequency changes through a sufficiently wide range, is great, merely by reason of the reflexions causing reinforcement or reduction of the strength of the received current. The theoretical least value of  $\rho$  is  $\frac{1}{2}$ , when  $R/Ln$  is vanishingly small, indicating a doubling of the amplitude of current. But as  $y$  increases the range of  $\rho$  gets smaller and smaller. After  $y=5$  it is negligible.

It is, however, the mean  $\rho$  that is of most importance, because the influence of terminal resistances is to lower the range in  $\rho$ , and to a variable extent. The value  $y=2.065$ , or, practically,  $Rl=2Lv$ , makes the mean  $\rho$  a minimum. As I pointed out in the paper before referred to, these fluctuations can only be prejudicial to telephony. In the present Note I have described how to almost entirely destroy them. The principle may be understood thus. Let the circuit be infinitely long first. Then its impedance to an intermediate impressed force alternating with sufficient frequency to make  $R/Ln$  small will be  $2Lv$ , viz.  $Lv$  each way. The current and potential-difference produced will be in the same phase, and in moving away from the source of energy they will be similarly attenuated according to the time-factor  $e^{-Rt/2L}$ . In order that the circuit, when of finite length, shall still behave as if of infinite length, the constancy of the impedance suggests to us that we should make the terminal apparatus a mere resistance, of amount  $Lv$ , by which the waves will be absorbed without reflexion.

That this is correct we may prove by my formula for the amplitude of received current when there is terminal apparatus, equation (19 b), Part V. "On the Self-Induction of Wires" (Phil. Mag. Jan. 1887). It is

$$C_0 = 2V_0 \left[ \frac{K^2 + S^2 n^2}{R'^2 + L'^2 n^2} \right] [G_0 G_1 e^{2Pl} + H_0 H_1 e^{-2Pl} - 2(G_0 G_1 H_0 H_1)^{\frac{1}{2}} \cos 2(Ql + \theta)]^{-\frac{1}{2}}.$$

Here  $C_0$  is the amplitude of received current at  $z=l$  due to  $V_0 \sin nt$  impressed force at  $z=0$ ;  $R'$  and  $L'$  the effective resistance and inductance per unit length of circuit;  $K$  and  $S$  the leakage-conductance and permittance per unit-length,

$$P \text{ or } Q = \left(\frac{1}{2}\right)^{\frac{1}{2}} \{ (R'^2 + L'^2 n^2)^{\frac{1}{2}} (K^2 + S^2 n^2)^{\frac{1}{2}} \pm (KR' - L'Sn^2)^{\frac{1}{2}} \};$$

$G_0, H_0$ , are terminal functions depending upon the apparatus

at  $z=0$ ;  $G_1, H_1$ , upon that at  $z=l$ ; the apparatus being of any kind, specified by resistance-operators, making  $R_0', L_0'$  the effective resistance and inductance of apparatus at  $z=0$ , and  $R_1', L_1'$ , at  $z=l$ .  $G_0$  is given by

$$G_0 = 1 + (R'^2 + L'^2 n^2)^{-1} [(P^2 + Q^2)(R_0'^2 + L_0'^2 n^2) + 2P(R'R_0' + L'L_0' n^2) + 2Qn(R_0'L' - R'L_0')],$$

from which  $H_0$  is derived by changing the signs of  $P$  and  $Q$ ; whilst  $G_1$  and  $H_1$  are the same functions of  $R_1', L_1'$  as  $G_0$  and  $H_0$  are of  $R_0', L_0'$ .

Now drop the accents, since we have only copper wires of low resistance (but not very thick) in question, and the terminal apparatus are to be of the simplest character.  $K, Sn$  will be vanishingly small practically, so take  $K=0$ . Next let  $R, Ln$  be small, and let the apparatus at  $z=l$  be a mere coil,  $R_1$ , of negligible inductance first. We shall now have

$$P = R/2Lv, \quad Q = n v,$$

and these make

$$G_1^{\frac{1}{2}} = \left(1 + \frac{R_1}{Lv}\right), \quad H_1^{\frac{1}{2}} = \left(1 - \frac{R_1}{Lv}\right).$$

Thus  $R_1 = Lv$  makes  $H_1$  vanish, whatever the length of line, and the terms due to reflexions disappear.

We now have

$$G_0 = \frac{V_0}{Lv} e^{-Rl/2Lv} \times G_0^{-\frac{1}{2}},$$

where  $G_0^{-\frac{1}{2}}$  expresses the effect of the apparatus at  $z=0$  in reducing the potential-difference there,  $V_0$  being the impressed force, and the value of  $G_0$  being unity where there is a short-circuit.

Now to show that  $R_1 = Lv$  makes the magnetic force of the receiver the greatest, go back to the general formula, let  $e^{-Pt}$  be small, and let the size of wire vary, whilst the size of the receiving-coil is fixed. It will be easily found, from the expression for  $G_1$ , that the magnetic force of the coil is a maximum when

$$R_1^2 + L_1^2 n^2 = \left( \frac{R^2 + L^2 n^2}{K^2 + S^2 n^2} \right)^{\frac{1}{2}},$$

where we keep in  $L_1$  the inductance of the receiver. Or, when  $R/Ln$  and  $K/Sn$  are both small,

$$(R_1^2 + L_1^2 n^2)^{\frac{1}{2}} = Lv,$$

or, as described,  $R_1 = Lv$  when the receiver has a sufficiently small time-constant. The rule is, equality of impedances.

We may operate in a similar manner upon the terminal



function at the sending end. Suppose the apparatus to be representable as a resistance containing an electromotive force, and that by varying the resistance we cause the electromotive force to vary as its square root. Then, according to a well-known law, the arrangement producing the maximum external current is given by  $R_0 = Lr$ , equality of impedances again. This brings us to

$$C_0 = \frac{V_0}{2Lv} e^{-Rl/2Lv};$$

as if the circuit were infinitely long both ways, with maximum efficiency secured at both ends.

Lastly, the choice of  $L$  such that  $Rl = 2Lv$  makes the circuit, of given resistance, most efficient.

In long-distance telephony using wires of low resistance, the waves are sent along the circuit in a manner closely resembling the transmission of waves along a stretched elastic cord, subject to a small amount of friction. In order to similarly imitate the electrostatic theory, we must so reduce the mass of the cord, or else so exaggerate the friction, that there cannot be free vibrations. We may suppose that the displacement of the cord represents the potential-difference in both cases. But the current will be in the same phase as the potential-difference in one case, and proportional to its variation along the circuit in the other.

We may conveniently divide circuits, so far as their signalling peculiarities are concerned, into five classes. (1) Circuits of so short length, or so operated upon, that any effects due to electric displacement are insensible. The theory is then entirely electromagnetic, at least so far as numerical results are concerned. (2) Circuits of such great length that they can only be worked so slowly as to render electromagnetic inertia numerically insignificant in its effects. Also some telephonic circuits in which  $R/Ln$  is large. Then, at least so far as the reception of signals is concerned, we may apply the electrostatic theory. (3) The exceedingly large intermediate class in which both the electrostatic and electromagnetic sides have to be considered, not separately, but conjointly. (4) The simplified form of the last to which we are led when the signals are very rapid and the wires of low resistance. (5) The non-distortional circuit, in which, by a proper amount of uniform leakage, distortion of signals is abolished, whether fast or slow. Regarded from the point of view of practical application, this class lies on one side. But from the theoretical point of view, the non-distortional circuit lies in the very focus of the general theory, reducing

it to simple algebra. I was led to it by an examination of the effect of telephones bridged across a common circuit (the proper place for intermediate apparatus, removing their impedance) on waves transmitted along the circuit. The current is reflected positively, the charge negatively, at a bridge. This is the opposite of what occurs when a resistance is put in the main circuit, which causes positive reflexion of the charge, and negative of the current. Unite the two effects and the reflexion of the wave is destroyed, approximately when the resistance in the main circuit and the bridge resistance are finite, perfectly when they are infinitely small, as in a uniform non-distortional circuit.

XXV. *The Geological Consequences of the Discovery of a Level-of-no-Strain in a Cooling Globe.* By T. MELLARD READE, C.E., F.G.S., F.R.I.B.A.\*

WHILE working out my theory of the Origin of Mountain-Ranges I became clearly convinced that there was a fallacy underlying the popular conception of the effect of secular cooling upon the crust of the earth. It is very extraordinary that the physicists who supported the hypothesis that mountain-chains had been ridged up through the outer uncontracting crust following by gravitation the shrinking nucleus, were content to leave the idea in this very vague form.

It is obvious, when once pointed out, that on this hypothesis it is only the actual surface of the earth that does not contract. As the isotherms sink deeper in the progress of secular refrigeration, each zone below the surface in the solid crust must contract horizontally proportionately to the lowering of the temperature. At a certain depth the rate of this horizontal, or in other words circumferential, contraction must equal the mean rate of the radial contraction of the sphere. At this particular zone—which varies in depth as the time—I showed† that there is no strain; while all of the crust above it is in compression, and below it all the cooling matter is contracting. After fully realizing this conception, a few calculations convinced me that on the hypothesis that our globe has cooled in this way during all geological time, the level-of-no-strain, as Mr. Fisher aptly names it, could not at the present time be many miles deep below the surface. All this seemed so very clear to me that I

\* Communicated by the Author.

† Chap. xi. 'Origin of Mountain Ranges.'

confess I was surprised at this particular chapter escaping the notice of my reviewers, who, as a rule, dealt very fairly and openly with the ideas put forth, so that I was fully prepared for a long continuance of indifference towards this interesting problem.

The able papers of Mr. Davison and Prof. Darwin, and the exhaustive mathematical investigation of Mr. Osmond Fisher\* have agreeably dispelled this fear.

It is very satisfactory that each of these mathematicians, though working on an independent basis, and all holding different views as to the geological consequences of the discovery, are in singularly close agreement in numerical results. Mr. Davison puts the level-of-no-strain after the lapse of 174 million years since consolidation at 5 miles; Prof. Darwin at 2 miles in 100 million years; and Mr. Fisher, taking the present temperature gradient as  $1^{\circ}$  F. in 51 feet, arrives at the conclusion that the level-of-no-strain is now at a depth of 2.1361 miles. I may add that my own numerical results, computed in an entirely different manner, approximate closely to those of Mr. Fisher and Prof. Darwin.

My present intention is not, however, to dwell upon the interesting nature of these mathematical investigations of the properties of a cooling globe, but from a geological standpoint to speculate upon the results that flow from the discovery of the existence of a level-of-no-strain situated not many miles beneath our feet.

*Volcanic Energy.*—Mr. Mallet has based a complete theory of the origin of volcanic energy upon the heat developed by the crushing and compression of the rocks of the outer crust of the earth while following the contracting nucleus. Many objections to this theory have been pointed out from time to time by practical geologists, and I have dealt with the question elsewhere†. The existence of a level-of-no-strain within a few miles of the surface, if admitted, is absolutely fatal to Mallet's hypothesis. Not only is the number of cubic miles of rock‡ crushed quite insufficient to account for the necessary heat, but the limitation of the crushing to what I have called

\* "On the Distribution of Strain in the Earth's Crust," by C. Davison, Phil. Trans. of Royal Soc. 1887, p. 231. "Note on Mr. Davison's paper," by Prof. G. H. Darwin, Ibid. p. 242. "On the mean height of the Surface-elevations and other Quantitative Results of the Contraction of a Solid Globe through Cooling" (Phil. Mag., Jan. 1888), by Rev. O. Fisher.

† Pp. 3-5 and chap. xxi. 'Origin of Mountain-Ranges.'

‡ Mr. Fisher estimates the amount of rock displaced by compression during all geological time as equal to a spherical shell the diameter of the Earth and 19 feet thick.

the "shell-of-compression" entirely forbids the acceptance of this secondary theory of volcanic energy.

Taking the greatest estimate of the present depth of the level-of-no-strain, the seat of volcanic energy on Mallet's hypothesis would be limited to a depth of less than 5 miles. When we point out that lava rises up to and flows from orifices of volcanoes at a level in some cases of  $2\frac{1}{2}$  miles above the sea, it is obvious to anyone who has the slightest knowledge of mechanics that the locus of its origin in the earth's crust must be greatly below the highest estimate of the thickness of the shell-of-compression.

*Cores of Mountain-Ranges.*—The distinguishing characteristics of great mountain-chains is, as I have fully pointed out elsewhere, the presence of a central core of gneissic or granitic rock, frequently, as in the case of the Alps, taking the form of a series of ellipsoidal bodies throwing off on the flanks—not seldom with reversed dips—the sedimentary rocks through which they have been protruded.

This is also true of the great ranges of the Caucasus, the Pyrenæes, the Rocky Mountains, the Himalayas, the Andes; and I believe such gneissic cores will be found to distinguish every great range when sufficient observations have been made. The apparent exception of the Appalachian chain is, in my view, no exception at all; and I have already brought forward a considerable body of evidence to prove that the gneissic masses lying to the eastward of the great sedimentary folds is the original core of the range much denuded and deprived of the flanking sedimentary masses formerly existing to the eastward\*.

Whether this explanation be accepted or not, the gneissic and granitic rocks protruded through and often entangling in their folds the great sedimentary deposits which together admittedly constitute most mountain-chains, are a standing monument of the untruth (said in no offensive sense) of the contractional hypotheses of the origin of mountain-ranges.

These central cores could not have been forced up except accompanied by great lateral pressure, yet now we find, on accurately working out the contractional hypothesis, that the lateral pressure is, taking the highest estimate, zero at a depth of 5 miles, whereas, according to the estimates of accomplished geologists, the sedimentary strata through which the cores were protruded ranged from 5 to 10 miles thick. It is plain to demonstration that the lateral pressure that forced up the mountains could not reside in a shell-of-compression only 5 miles thick having a zero strain on the underside.

\* Pp. 34 and 35, 'Origin of Mountain-Ranges.'



*Colorado Plateaus.*—The contractional hypothesis is equally incapable of accounting for the elevation of the great monoclinal plateaus lying to the west of the Rockies. Indeed this has been insisted upon by American geologists, notably by Dutton and Gilbert, before the discovery of the level-of-no-strain. It is, therefore, unnecessary for me to dwell upon these arguments here, except to say that by this discovery the difficulties are increased beyond all possibility of explanation.

*Effects of Contraction on the Shell underlying the Shell-of-Compression.*—The bulk of the contracting portion of the earth's crust, even if the whole crust be estimated at not more than 30 miles thick, is so much in excess of the shell-of-compression, in addition to the much larger underlying contracting envelope, that I fully expect some reverse theory of mountain-building may be based upon it. Mr. Davison has already suggested that the ocean-basins are due to the localization of this contraction. Let me examine the problem in a fair spirit, and as fully as the limits of this paper will allow. In discussing the effects of contraction on the rocks of the crust underlying the shell-of-compression, I have said, "Practically tension could not take place, as the superincumbent strata would by vertical compression elongate the rocks at the zone of greatest contraction to fill the vacuities that otherwise would be created."\*

The depth of the zone at which compressive-extension commences will differ according to the nature and crushing strength of the rocks: but we may safely assume that all known rocks will be plastic under the superincumbent pressure at a depth of 10 miles †. It is, therefore, possible that somewhere in the solid crust between that depth and the surface, cavities might be formed by secular contraction, and shearing and faulting take place by the subsidence of overlying masses of crust into the plastic medium below. Irregularities of the surface of the sphere might arise in this way, as well as by inequalities in the amount and mode of shrinkage of the whole of the underlying contracting body or shell. It needs, however, but little consideration to see that such orographic changes of the surface would tend generally in the same direction; depressions once commenced would increase in depth by a continuation of the contraction which initiated them; while such elevations as might originate in the much smaller shell-of-compression would tend to increase in height and in number also. But while the agencies invoked in our

\* 'Origin of Mountain-Ranges,' p. 125.

† Ibid. 91.

hypothesis are tending to corrugate the surface of the globe, subaerial denudation is, *pari passu*, levelling all the irregularities that appear above the waters of the ocean, and sedimentation in varying degrees at different localities is more or less effacing the irregularities below the waters.

It would be an interesting calculation to approximately work out the relative magnitudes of these opposite actions, but such is not my present intention. It will be more to the purpose to enquire whether such a process—deductively arrived at—is in correspondence with what takes place in nature. The geological standpoint is an entirely different one to that of the deductive reasoner. The geologist is prepared to question any physical dogma otherwise called a “law,” no matter how high the authority on which it may stand, if observation leads him in an opposite direction. It is, however, in this branch of enquiry by the application of the two methods, the inductive and the deductive and their correspondence, that the most important and far-reaching truths are eventually discovered.

I can quite understand a contractionist, in view of the discovery of the level-of-no-strain, giving up the position that mountain-ranges have originated in the shell-of-compression, yet contending that the other irregularities of the earth's surface have originated in the differential movements of the underlying contracting shell. He would probably point to such a persistence of movement of subsidence, though only deductively arrived at, as another proof of the permanence of oceans and continents.

When, however, as geologists we go to Mother Earth and ask her to yield up her secrets, what does she answer?

It seems pretty safe to assert anything of the abysmal depths of the ocean, for we know little of the ocean-bottom excepting what is yielded by a few scrapings of the dredge and the number of fathoms read off on the sounding-line. When, however, we investigate that which is more within our reach, we find that everywhere on the globe there has been in progress a constant flux and reflux of elevation and subsidence, and it would be difficult to say which has been, on the whole, of the greater magnitude. It is but lately that Dr. Guppy has shown us that there are on the Solomon Islands soft foraminiferous rocks which, according to Murray and Brady, represent deep-sea deposits laid down in water from 1800 to 2000 fathoms deep. These deposits in many cases lie upon old denuded volcanic rocks, and are overlain with a capping of hard coral rock.

These deep-sea rocks are, in the case of Ugi and Treasury

Islands, themselves folded ; a most remarkable fact, for which we are indebted to Dr. Guppy's intrepidity and love of science. Assuming the correctness of all the facts made known by Dr. Guppy, they appear to me inconsistent with the assumption that the deposits took place on submerged volcanic peaks, for it is difficult to see how folding could originate in such a position by any cause now known. But in any case it points to an elevation of the sea-bottom, estimated by Dr. Guppy at 12,000 feet\*.

In reading the accounts of various observers scattered over the face of the globe, nothing has struck me more than the universality of the evidences—side by side—of recent depression and recent elevation in each and every country. It would be wearisome to recount them here. We know that the great continental masses are built up of sedimentary rock-systems, some of them reaching, in the locus of mountain-ranges, an estimated thickness of ten miles. Now let us see what this means. It indicates that the crust of the earth on these areas has been bent to a depth double that of the deepest of the deep-sea soundings.

Yet these very deposits now constitute the highest mountain-ranges! With these evidences of flux and reflux before our eyes, it seems really useless to invoke as an explanation the aid of a secular change tending to act more or less constantly in one direction. The investigation of the properties of a cooling globe and the discovery of the existence of a level-of-no-strain only a few miles beneath our feet, have greatly helped to clear the ground for the reception of a theory of mountain-formation which takes more fully into account the actualities of nature.

It may, however, be desirable, in the light of these recent discoveries, to try and trace by geological or other observations whether any features of the earth are directly attributable to secular cooling. Such would be a difficult inquiry, superimposed as I believe the effects will be found to be (if found at all), upon much more pronounced features due to other agencies.

Here for the present I must leave the subject, perhaps to return to it at a future time.

\* 'Geology of the Solomon Islands' (1887). "Observations on the recent Calcareous Formations of the Solomon Group made during 1882-4 by H. B. Guppy, M.B., F.G.S.," Trans. Roy. Soc. of Edin. 1885, p. 545.

XXVI. *On the General Laws of Brightness of Images.*

By Prof. J. D. EVERETT, F.R.S., Queen's College, Belfast\*.

I AM not acquainted with any optical Treatise which contains a general investigation of the brightness of an image seen by rays which have traversed a medium of continuously varying index. The only discussion of the subject that I have met with is contained in chapter xii. of Clausius's 'Mechanical Theory of Heat.' That chapter is entitled "On the Concentration of Rays of Light and Heat," and the main purpose of it is not optical. I have found that its optical results can be obtained by a much simpler method, as set forth in the present paper. The results themselves are probably familiar to leading physicists, but they certainly are not so well known as they deserve to be.

The particular case in which all the refractions and reflexions take place at coaxial spherical surfaces with nearly normal incidence is treated in my edition of 'Deschanel' (see especially footnote to § 768 of the earlier, or § 1037 of the later, editions), and had been previously treated by a different method in Helmholtz's 'Physiological Optics' (pp. 171-175).

The mode of investigation here adopted has been suggested partly by Clausius's discussion above referred to, partly by an investigation of Kirchhoff's in Poggendorff's *Annalen*, vol. cix. pp. 284-287, to which Clausius refers, and partly by § 334 (2nd edition) of Thomson and Tait's 'Natural Philosophy.'

We shall begin by establishing the following

*Theorem on Apparent Size.*

*If two small equal plane areas  $A_1, A_2$  are so placed that a ray from the centre of one to the centre of the other is perpendicular to them both, the two solid angles formed by rays at the centre of each which come from the circumference of the other are inversely as the squares of the absolute indices of refraction at the centres at which they are formed. That is, if  $\mu_1 \mu_2$  be the indices at the centres of  $A_1 A_2$ ,  $\omega_1$  the angle formed at the centre of  $A_1$  by rays from the circumference of  $A_2$ , and  $\omega_2$  the solid angle formed at the centre of  $A_2$  by rays from the circumference of  $A_1$ , then will*

$$\mu_1^2 A_1 \omega_1 = \mu_2^2 A_2 \omega_2. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

As a particular case, *if the two indices of refraction are equal, as well as the two areas, the two solid angles will be equal; in other words, the apparent size of the first area as seen from*

\* Communicated by the Author.



the second is the same as that of the second as seen from the first.

*Proof.*

Let  $T$  denote the time of propagation of light from a point  $xyz$  to a point  $\xi\eta\zeta$ , or from the latter point to the former. If  $\delta s$  be an indefinitely short line drawn from the point  $xyz$  at an inclination  $\theta$  to the forward direction of a ray from  $\xi\eta\zeta$ , the increase in  $T$  will obviously be the same as for a line  $\delta s \cos \theta$  drawn along the ray. Hence, if  $v$  denote the velocity of light at  $xyz$ , and  $\mu$  the absolute index of refraction at the same point, we have  $dT = \frac{1}{v} \delta s \cos \theta$ , or

$$\frac{dT}{ds} = \frac{\cos \theta}{v} = \frac{\mu \cos \theta}{V}, \quad . \quad . \quad . \quad . \quad (2)$$

where  $V$  denotes the velocity *in vacuo*.

Take the centre of the small area  $A_1$  as origin of the rectangular coordinates  $xyz$ , and the centre of  $A_2$  as origin of  $\xi\eta\zeta$ . Let the axes of  $z$  and  $\zeta$  be tangential to the ray, and reckoned positive in the outward directions. The axes of  $xy$  and of  $\xi\eta$  will therefore be in the planes of  $A_1$  and  $A_2$  respectively, and we shall choose them so as to satisfy the two conditions

$$\left[ \frac{d^2 T}{dx d\eta} \right] = 0, \quad \left[ \frac{d^2 T}{dy d\xi} \right] = 0, \quad . \quad . \quad . \quad . \quad (3)$$

the brackets signifying that the six variables  $x, y, z, \xi, \eta, \zeta$  are to be put equal to zero after the performance of the differentiations.

To express the solid angle formed at the centre of  $A_1$  by rays from the circumference of  $A_2$ , suppose tangents of unit length, forming prolongations of the rays, to be drawn from the centre of  $A_1$ . The solid angle (being small) will be numerically equal to the base of the cone or pyramid enclosed by the tangents, or to the projection of this base on the plane of  $xy$ . Let  $x'$  and  $y'$  be the coordinates of the projection of the extremity of one of these unit-tangents, then the solid angle will be equal to the area traced by the point  $x'y'$  in the plane of  $A_1$ , while the point  $\xi\eta$ , from which the ray proceeds, moves round the circumference of  $A_2$ . The coordinates  $x'$  and  $y'$  are obviously equal to the cosines of the angles which the ray makes with the axes of  $x$  and  $y$ .

Hence, putting  $\cos \theta$  in (2) successively equal to  $x'$  and  $y'$ , we have

$$x' = \frac{V}{\mu_1} \frac{dT}{dx}, \quad y' = \frac{V}{\mu_1} \frac{dT}{dy}, \quad . \quad . \quad . \quad . \quad (4)$$

$\mu_1$  denoting the value of  $\mu$  at the centre of  $A_1$ .

Again, since  $x'$  and  $y'$  are functions of the two independent variables  $\xi$  and  $\eta$ , and vanish with them, we have, to the first order of small quantities,

$$x' = \left[ \frac{dx'}{d\xi} \right] \xi + \left[ \frac{dx'}{d\eta} \right] \eta, \quad y' = \left[ \frac{dy'}{d\xi} \right] \xi + \left[ \frac{dy'}{d\eta} \right] \eta. \quad (5)$$

But by (4),

$$\frac{dx'}{d\xi} = \frac{V}{\mu_1} \frac{d^2T}{dx d\xi}, \quad \frac{dx'}{d\eta} = \frac{V}{\mu_1} \frac{d^2T}{dx d\eta}. \quad (6)$$

Thus equations (5) reduce, by the help of (3), to

$$x' = \frac{V}{\mu_1} \left[ \frac{d^2T}{dx d\xi} \right] \xi, \quad y' = \frac{V}{\mu_1} \left[ \frac{d^2T}{dy d\eta} \right] \eta. \quad (7)$$

Hence  $x'$  has a constant ratio to  $\xi$ , and  $y'$  a constant ratio to  $\eta$ . The product of these two ratios will be the ratio of the area traced by the point  $x'y'$  to the area traced by  $\xi\eta$ , that is, it will be the ratio of the solid angle  $\omega_1$  to the area  $A_2$ . We have accordingly

$$\omega_1 = \left( \frac{V}{\mu_1} \right)^2 \left[ \frac{d^2T}{dx d\xi} \right] \left[ \frac{d^2T}{dy d\eta} \right] A_2. \quad (8)$$

By applying similar reasoning to rays from the perimeter of  $A_1$  to the centre of  $A_2$ , we shall find (with similar notation)

$$\xi' = \frac{V}{\mu_2} \left[ \frac{d^2T}{dx d\xi} \right] x, \quad \eta' = \frac{V}{\mu_2} \left[ \frac{d^2T}{dy d\eta} \right] y, \quad (9)$$

$$\omega_2 = \left( \frac{V}{\mu_2} \right)^2 \left[ \frac{d^2T}{dx d\xi} \right] \left[ \frac{d^2T}{dy d\eta} \right] A_1. \quad (10)$$

By comparison of (8) and (10) we have

$$\mu_1^2 A_1 \omega_1 = \mu_2^2 A_2 \omega_2. \quad (1)$$

The rays considered in the foregoing proof may undergo any amount of either gradual or abrupt bending by refraction, and may be reflected any number of times; but there must be no abrupt difference between the histories of rays which come from consecutive points.

We now proceed to apply this theorem to the investigation of brightness.

The brightness of an object as seen from any point is measured by

$$\frac{q}{\Lambda \omega}, \quad (11)$$

$\Lambda$  denoting a small area at the point, sensibly perpendicular to the rays which reach it from the object,  $\omega$  a small solid angle of arbitrary magnitude, and  $q$  the quantity of light from the object which converges to  $\Lambda$  (and also diverges

from A if not stopped) in pencils of magnitude  $\omega$ . The justness of this measure is obvious from the fact that if we halve either A or  $\omega$  we shall halve the quantity of light that will come into consideration.

When the small area A is taken on the surface of the object itself, the value of the expression (11) is called the *intrinsic brightness* of the object.

In the case of the two small areas which we have been discussing, let  $q$  denote the quantity of light sent by  $A_2$  on the road to  $A_1$ . If none of it is lost on the road, it all reaches  $A_1$ ; thus the same quantity of light which leaves  $A_2$  in pencils of angle  $\omega_2$  reaches  $A_1$  in pencils of angle  $\omega_1$ . Let  $I_2$  denote the intrinsic brightness of  $A_2$ , and  $I$  its apparent brightness as seen from  $A_1$ ; then

$$I = \frac{q}{A_1 \omega_1} = \frac{A_2 \omega_2}{A_1 \omega_1} \frac{q}{A_2 \omega_2} = \left( \frac{\mu_1}{\mu_2} \right)^2 \frac{q}{A_2 \omega_2} = \left( \frac{\mu_1}{\mu_2} \right)^2 I_2. \quad (12)$$

By supposing  $\mu_2$  equal to  $\mu_1$  we obtain the following theorem. *If the eye and the object are in media of the same index, and no light is stopped on its way from the object to the eye, the apparent brightness is equal to the intrinsic brightness, notwithstanding any refractions or total reflexions that the rays may have undergone between the object and the eye.*

If light is lost on the way, the above expression for the apparent brightness must be multiplied by a coefficient  $k$  less than unity, which, by well-established laws of Optics, is the same for rays going from  $A_2$  to  $A_1$  as for those going from  $A_1$  to  $A_2$  (for the same kind of light). If  $I_2$  and  $I_1$  are the intrinsic brightnesses of the two areas, their apparent brightnesses when each is seen from the other will be

$$k \left( \frac{\mu_1}{\mu_2} \right) I_2, \quad \text{and} \quad k \left( \frac{\mu_2}{\mu_1} \right) I_1. \quad . \quad . \quad . \quad (13)$$

When an observer sees the area  $A_2$ , the pupil of his eye may be taken as the area  $A_1$ ; the foregoing computations of apparent brightness accordingly assume that all parts of the pupil receive rays from every part of  $A_2$ . If part of the pupil be covered, the apparent brightness of the object will be diminished; and a similar diminution will occur when the rays from the object are collected into a beam of such small section as not to fill the pupil. This is the cause of the falling off of light which is observed in the use of high magnifying-powers with optical instruments.

In the case of light sent out obliquely from the surface of an object, the above investigations will remain applicable if

we understand by  $\Lambda_2$  the projection of this surface on a normal section of the beam.

When  $\Lambda_1$  is the image of  $\Lambda_2$  the "theorem on apparent size" requires modification, for the centre of  $\Lambda_1$  will no longer receive one ray from each point of  $\Lambda_2$ , but will receive all its rays from one point only, which we shall regard as the centre of  $\Lambda_2$ . Let a section orthogonal to all the rays which go from the centre of  $\Lambda_2$  to the centre of  $\Lambda_1$  be taken anywhere in the intermediate region, but not so as to coincide with a surface at which there is an abrupt change of direction in any of the rays. For instance, if the image is formed by a lens, the section may be taken either in the substance of the lens or in the external medium, but must not coincide with a face of the lens. Divide this section into parts comparable in size with  $\Lambda_1$  or  $\Lambda_2$ , and let  $\Lambda_3$  denote the area of one of these parts. Also let  $\mu_3$  denote the index of refraction at the centre of  $\Lambda_3$ . Since  $\Lambda_1$  is the image of  $\Lambda_2$ , the rays which go from the perimeter of  $\Lambda_2$  to a fixed point of  $\Lambda_3$  will go on to the perimeter of  $\Lambda_1$ ; thus  $\Lambda_2$  and  $\Lambda_1$  will subtend the same solid angle as seen from  $\Lambda_3$ , call it  $\omega_3$ . Hence, by applying (1) first to  $\Lambda_1$  and  $\Lambda_3$  and then to  $\Lambda_3$  and  $\Lambda_2$ , we have

$$\mu_1^2 \Lambda_1 \omega_1 = \mu_3^2 \Lambda_3 \omega_3 = \mu_2^2 \Lambda_2 \omega_2, \quad . . . \quad (14)$$

$\omega_1$  and  $\omega_2$  denoting the solid angles formed at  $\Lambda_1$  and  $\Lambda_2$  by rays which traverse the perimeter of  $\Lambda_3$ .

Equations (14) show that the ratio of  $\omega_1$  to  $\omega_2$  is the same for all the portions such as  $\Lambda_3$  into which the intermediate section is divided. Let  $\Omega_1 \Omega_2$  be the whole solid angles at  $\Lambda_1 \Lambda_2$  formed by the aggregate of all the rays which go from  $\Lambda_2$  to  $\Lambda_1$ .  $\Omega_1$  is the sum of all the partial angles  $\omega_1$ , and  $\Omega_2$  is the sum of all the partial angles  $\omega_2$ ; hence we have

$$\mu_1^2 \Lambda_1 \Omega_1 = \mu_2^2 \Lambda_2 \Omega_2, \quad . . . \quad (15)$$

a result which is similar in form to (1).

The rays which go from  $\Lambda_2$  to  $\Lambda_1$  diverge from  $\Lambda_2$  in the solid angle  $\Omega_2$ , and converge to  $\Lambda_1$  in the solid angle  $\Omega_1$ . Let  $Q$  be the whole light sent from  $\Lambda_2$  on the way to  $\Lambda_1$ , and  $kQ$  the portion of it which actually reaches  $\Lambda_1$ , then the intrinsic brightness  $I$  of the real image  $\Lambda_1$  will be

$$I = \frac{kQ}{\Lambda_1 \Omega_1} = \frac{k\Lambda_2 \Omega_2}{\Lambda_1 \Omega_1} \frac{Q}{\Lambda_2 \Omega_2} = k \left( \frac{\mu_1}{\mu_2} \right)^2 \frac{Q}{\Lambda_2 \Omega_2} = k \left( \frac{\mu_1}{\mu_2} \right)^2 I_2, \quad (16)$$

as in (13). When the pupil of the eye is placed at the image, the brightness of the field of view will be identical with this intrinsic brightness of the image, if the pupil is filled. The apparent brightness of  $\Lambda_2$  as seen from  $\Lambda_1$  will then be unaffected by the circumstance that  $\Lambda_1$  is the image of  $\Lambda_2$ .



XXVII. *On the Tetravalency of Oxygen; with Remarks on the Varying Valency of the Elements and the Present Chemical Aspect of the Valency Problem.* By J. F. HEYES, M.A., F.C.S., F.R.G.S., of Magdalen College, Oxford\*.

THE actual state, or rather phase, of philosophic Chemistry presents us with a remarkable paradox. Simultaneous with the discovery and chemical analysis of countless new compounds and many new elementary or non-resolved substances during the present century, there have emerged a series of views on the constitution of matter and the correlation of material forces which will for many centuries to come mark the greatest epoch in scientific philosophy. Although this molecular theory of matter has been chiefly developed on the physical and dynamical side, the contributions from the chemical side of the hedge of this great field of research have been very striking—such, in fact, as to stimulate the work of our greatest physicists. Scientific chemists, it may be said, have established the law of the conservation of mass; physicists, that of energy: now both are united in searching out the nature and influences of the masses and forces which are at work in the material world of chemical substances. But while the smaller limit, so to speak, of physical analysis is the molecule, it forms the upper limit of chemical analysis and synthesis. The main studies of the physicists are inter-molecular, those of the chemists intra-molecular phenomena. From this point of view, strict Chemistry begins where Physics ends; but, viewed historically, a growing interaction must be confessed, and, indeed, a friendly struggle has begun for the wells of the border-land which the one side calls molecular physics and the other physical chemistry. Both are peculiarly interested and puzzled just now in what are somewhat curiously termed “molecular compounds” or aggregations. Undoubtedly the trouble is chiefly and naturally on the chemical side; and, in the conventional system of shorthand or formulæ which the chemist uses to express (1) his *facts* about their composition and (2) his *views* about their constitution, it may be said to take the form of a discussion as to the meaning of certain dots and dashes with which our new text-books are plentifully (and to many students, I fear, misleadingly) sprinkled.

If, therefore, in calling attention to the question of the

\* Communicated by the Author. Read before the Ashmolean Society, Oxford, 7th November, 1887.

valency of an element, and the so-called atomicity of atoms, the ordinary language of chemical theory is used, it is not because I have any belief in an ultimate solution of the problems on the chemical side, but because, as the problems now stand, the conditions of the great mystery of chemical affinity can best be attacked, and perhaps by chemists can only be attacked, by facing the problems of what has been termed atomicity, valency, and quantivalence.

There is so much confusion of language on this subject that I must beg leave to make a very elementary start with our three historic typical compounds— $\text{HCl}$ ,  $\text{H}_2\text{O}$ ,  $\text{H}_3\text{N}$ .

Let us most carefully recognize the facts conveyed in these formulæ first; for these facts, if fully ascertained, must remain good when our transitional conceptions of them "have faded into the infinite azure of the past."

(1) There are the facts of definite composition by weight and volume. The three gas formulæ weights occupy the same volume.

(2) Experience, aided by comparison of composition, has led us to the laws and facts of definite and multiple proportion.

(3) The latter can only, as far as we can see, be *explained* in terms of the old atomic theory.

(4) Referring to this theory, Dalton showed us it was possible to obtain the *ratio-weights of the atoms*.

The symbol O in our formulæ has therefore more than one interpretation. It stands for (1) oxygen stuff, (2) an atom of oxygen, and (3) for a number. Here is the material for a confusion of thought which is observable in the history of our science. This has been increased by the natural coincidence of our selection of the lightest stuff known (hydrogen) as the unit to which we refer (1) densities (that is, the ratio-masses of equal volumes), which are absolute facts, (2) atomic weights (that is, the most probable ratio-masses of atoms whose volumes are unknown), which are relative facts. Thus

$$\frac{\Delta_o}{\Delta_h} = \frac{16}{1} \text{ expresses pure fact;}$$

but

$$\frac{O}{H} = \frac{16}{1} \text{ expresses well-grounded belief,}$$

based upon the cumulative probability resulting from the consideration of many physical and chemical data. It is to be noted, moreover, that the question of the most probable value  $O=15.96$ , accepted by many chemists, *versus* the exact integer 16, does not, so far as I can see at present, affect con-

siderations of valency or atomicity. But it may subsequently affect the questions as to the actual number of hydrogen and chlorine atoms in a molecule of hydrogen chloride, and the discussion of what Professor Armstrong calls "residual affinity." The atomic weight—that is, ratio-atomic weight—of oxygen is in this sense 16. But it is well to bear in mind that the number *can*, if necessary, be taken in the more natural sense of a "combining proportion" or combining weight, and that this number 16 is not indissolubly bound up with the statement that the oxygen atom is diatomic or divalent, as is sometimes supposed. Indeed, Professor Odling, in his historic paper\* on the weight of the oxygen atom, demonstrated that  $O=16$ , and not 8 as then usually taken *if*  $N=14$ , as was universally accepted *whether in a strictly atomic sense or not*.

Taking, then, equal volumes of these three gases, it is a fact that we have one, two, and three unit weights and volumes combined with the chemical unit weights of Cl, O, and N. Apart even from Avogadro's law, the chemist would, adopting the atomic theory, say that "atoms" of chlorine, oxygen, and nitrogen respectively combined with one, two, and three atoms of hydrogen *in these compounds*; and hence that these atoms were mono-, di-, and tri-atomic respectively. In the developments of the substitution- and type-theories, and above all in the development of the hydrocarbon theory, based upon the very strikingly constant "tetratomicity of carbon," this theory has been so useful that there has been a strong school of chemists to maintain this atomicity as a constant property—"a property inherent in the nature of atoms."†

The word "atomicity" is thus naturally associated with some inherent and occult property of the atom or some force centering from it. Since it has become more generally recognized that the atomicity of many elements seems to vary when either (1) the chemical conditions of A and B are altered, or (2) other elements are chemically associated with A or B, the word "atomicity" has wisely gone out of fashion. More strictly speaking, it is the phrase "the *atom* of oxygen is diatomic" which is disappearing, because physicists (*e. g.* Prof. J. J. Thomson) write about "diatomic molecules," meaning molecules which contain two chemical atoms. Thus it is now said that "the *molecule* of oxygen is diatomic," and that "the atom of oxygen is divalent," or, most briefly, that "oxygen is a dyad." The shorthand form remains distinct; it is  $O_2$  for the diatomic molecule, but  $O''$  or  $O^{\text{II}}$  for the divalent atom.

\* Journ. Chem. Soc. London, vol. xi.

† Wurtz, 'Atomic Theory,' p. 224.

Now the development of the chemistry of the carbon compounds has shown us that not merely is there chemical contiguity of some sort between the atoms in a molecule (*e. g.*  $\text{H}_2\text{O}=\text{HHO}$  or  $\text{HOH}$ ), but that there are special and preferable contiguities between certain atoms or (for brevity's sake, and sometimes provisionally for want of knowledge) groups of atoms. These special associations, often termed "atomic linkings," are seen to keep together through many formative and transformative reactions; and they are indicated in our so-called constitutional formulæ, where these special associations are indicated by so-called "links" or "bonds." The extraordinary progress in complex organic synthesis, the thousands of syntheses which have resulted in so few years of labour in the great and, on the whole, wonderfully systematic and connected text-books of organic chemistry, compel us, I think, to retain the notion of "links" or "bonds," or, as I prefer to call them, "valencies," in writing these constitutional formulæ. Not merely, then, do we write divalent oxygen as  $-\text{O}-$  but also as  $\text{O}=\text{}$  in our constitutional formulæ, according as we interpret, and fairly consistently interpret, our reactions. Thus, in the case of water, alcohol and ether, with their analogues, do we find  $-\text{O}-$ , but in ketones and aldehyds  $=\text{O}$ , and in the organic acids both aspects of divalent oxygen. Similarly the nitrogen atom may be directly specially associated with three other atoms (as in ammonia,  $\text{N}'$ ), or it may not; but this does not necessarily interfere with its usually trivalent, and perhaps occasionally pentavalent, character.

It is in some such sense as this that I believe that oxygen is occasionally, and probably in more cases than is usually supposed, tetravalent. Pattison Muir\* gives what is probably the most generally accepted definition:—"The atom of oxygen can directly act on and be acted on by two other atoms in a molecule;" but adds, "in some molecules there are only two atoms, one of which is oxygen." This of course refers to the well-known gaseous puzzles,  $\text{NO}$ ,  $\text{CO}$ , compared with the standard molecule  $\text{HCl}$ . Perhaps it would be more cautious to say, instead of "act on," "are directly chemically associated with," since it is this *statical* aspect which our formulæ indicate. It is not therefore the case that a tetravalent element is necessarily associated with four monovalent elements. Where this is the case, as in the hydrides, halides, and methides, I should propose that the old word "tetratomic" be used. It is here, in fact, that the idea of the inherent

\* Muir, 'Thermal Chemistry,' p. 67.



property of the atom is most striking. We might usually confine this notion of the *thing* or *force* to "atomicity," and reserve the idea involved in the *number* or *ratio* to "valency." I propose that we should always look upon valency as a *number* and atomicity as a *property*, but both still to be investigated. That the valency of all known atoms is a small integer, and most probably 1, 2, 3, 4, 5, or 6, is surely one of the best assured facts in the present state of chemical knowledge. It is a separate question whether in particular compounds the valency of a selected atom may vary within the limits of these numbers. It is also a separate question what this number really means. But I would particularly point out that this valency is apparently independent of the discussions on and decimal revisions of our atomic weights. For valency as a number must be a ratio and referred to monovalent H'; and practically we have referred the valency of all atoms to either monovalent H', or to that unit weight of chlorine which combines with the unit weight of hydrogen in the H'Cl'. Unfortunately the word equivalent has had and has several meanings attached to it. But it is most convenient to take it as a weight, *viz.* that weight of the non-metal which combines with unit weight of the gaseous metal hydrogen, or that weight of a metal which combines with the chemical unit weight of the gaseous non-metal chlorine.

Our first most obvious and useful definition of valency is, first, that it is the numerical ratio,

$$\frac{\text{atomic weight}}{\text{equivalent weight}};$$

*e. g.* (1) valency of antimony (in antimony trichloride) is

$$\frac{120}{40} = 3, \text{ i. e. Sb}''',$$

(2) valency of oxygen (in steam) is

$$\frac{16}{8} \quad \text{or} \quad \frac{15.96}{7.98} = 2, \text{ or O}''.$$

As some elements have more than one equivalent weight, but can only have one atomic weight\*, it is not surprising that the valency of an element may be a varying number. But although this is really a practical rule in the case of almost all the metals, we meet with difficulties in the non-metals. Thus, on the hydrogen standard, this kind of valency of oxygen is II. and I. (*cf.* mercury and chlorine, giving II. and I. also†),

\* Dr. W. Crookes has recently speculated that the atomic weights of individual atoms may slightly vary, assuming that they contain the hypothetical *protyle*.

† Usually formulated Hg'' and (Hg<sub>2</sub>)'.

but the oxygen chlorides give 2,  $\frac{2}{3}$ , and  $\frac{1}{2}$ , which ratios are those of 12, 4, and 3. It would be illogical to infer the multivalency of oxygen from these figures, but they seem to support clearly the *integral* character of valency. On this account I would venture to deprecate the use, except with extreme caution, of such phrases as "residual affinities" or "residual valencies," at any rate so long as we fully and strictly accept the fundamental laws of definite, constant, and multiple proportions. Whatever may emerge as the precise meaning of that well-known little . in those formulæ where the joint influence of reactions and of notions will neither consent to the distant chemical acquaintance of +, nor the intimate familiarity of the valence-link — (so prematurely thrust upon us in inorganic text-books), it would nevertheless seem clear that we are observing the action of *integral units of some sort*, and not residual or small decimal quantities of chemism, affinity, or attraction—whichever our mysterious force be called.

It is in such apparently simple cases as the oxides of chlorine and nitrogen that the combined doctrines of valency and linking seem to break down, or are at least not obvious. Here is the great paradox in our constitutional formulæ, even from the low standpoint of reactions' shorthand. The doctrines have been applied with marvellous success to analysis, synthesis, and prevision of thousands of organic compounds, including dozens of liquids which, although they give the same analytical and vapour-density results, are absolutely distinct in their properties and reactions. And, basing their work on the idea of the geometrical stability of a molecular system containing strictly tetravalent atoms, Van 't Hoff and others have gone still further. Yet in the apparently simple molecules of inorganic chemistry, we are still in a state of comparative chaos. At least we may say that, whereas in organic chemistry the young student finds the constitutional formulæ the most helpful shorthand-memoranda possible of reactions and relationships, the beginner in inorganic chemistry is usually disastrously affected, if not hopelessly muddled, by the presumed necessity of writing these so-called graphic (?) formulæ, in the case, for instance, of the oxides of nitrogen, chlorine, and manganese, or even for what are commonly called ammonia, hydrochloric acid and nitric acid.

In the case of hydrogen dioxide,  $\text{H}_2\text{O}_2$ , the only reasons assigned, so far as I know, for the usual formula  $(\text{OH})_2$  or

$$\begin{array}{c} \text{O} - \text{H} \\ | \\ \text{O} - \text{H} \end{array} \text{ or } \text{H} - \text{O} - \text{O} - \text{H},$$

are (1) that it is "free hydroxyl,"

(2) that it is obtained from  $\text{Ba} \begin{array}{c} \diagup \text{O} \\ | \\ \diagdown \text{O} \end{array}$ , and (3), in the words

of Wurtz (*l.c.*):—"If  $\text{O}=\text{O}$  represents a saturated couple, the symbol  $-\text{O}-\text{O}-$  will represent a couple which is unsaturated and capable of attaching, for example, two atoms of hydrogen. *This conception explains* the constitution of hydrogen peroxide,  $\text{H}-\text{O}-\text{O}-\text{H}$ ."

Now this is one of the most numerous class of bodies whose true molecular weight we have at present no means of ascertaining, or at least of comparing with gaseous molecules. It is, like  $\text{BaO}_2$ ,  $\text{PbO}_2$ ,  $\text{MnO}_2$ ,  $\text{SnO}_2$ ,  $\text{SiO}_2$ , *versus*  $\text{CO}_2$ , simply a chemical unit formula. As such formulæ are not strictly comparable like our two-volume formulæ, the chief, if not the only, use of constitutional formulæ in such cases should surely be to explain or indicate reactions, and thus to contribute something from the purely chemical standpoint to the solution of the problems of valency. It can scarcely be said

that  $\begin{array}{c} \text{OH} \\ | \\ \text{OH} \end{array}$  or  $\text{Ba} \begin{array}{c} \diagup \text{O} \\ | \\ \diagdown \text{O} \end{array}$  are, on these grounds, useful formulæ.

There is no chemical equality, so to speak, in the oxygen atoms, and no suggested explanation of why some peroxides have what is technically termed "available oxygen" and others have not.

One chief stumbling-block in our theories of quantivalence seems to be due to an apparently unanimous faith in the constant divalency of oxygen, as well as the constant monovalency of the halogens. This has been curiously illustrated in the adoption by many chemists of extreme views in two directions. Either the valency has been very largely varied between 1 and 7 in the case of the halogens, or the valency-power or atomicity of the multivalent metals has been largely increased. The late Professor Wurtz advocated the first view, and Professor Williamson's address at York furnishes an instance of the second standpoint. Certainly, rather than recognize so many valency powers\* for the non-metals, it

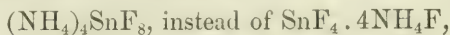
\* Thus Wurtz gave to chlorine the valencies 1, 3, 4, 5, and 7, and to nitrogen 1, 2, 3, 5. Heptavalent chlorine is also suggested by several philosophers, especially in connexion with the periodic arrangements of the elements. In such cases, at least equal weight must be given to aluminium and gallium as trivalent in the periodic series, whereas the tetravalency is constantly asserted, based on the two-volume molecular

formula,  $\text{M}_2^{\text{IV}}\text{Cl}_6$ . I have suggested the alternative,  $\text{Al} \begin{array}{c} \diagup \text{Cl}=\text{Cl} \\ | \quad \diagdown \\ \text{Cl}-\text{Cl}=\text{Cl} \\ | \quad \diagup \\ \text{Cl}=\text{Cl} \end{array} \text{Al}$ , in

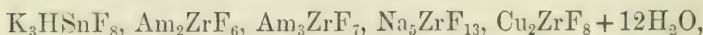
these and similar cases as more probable than the usually assumed

metallic nucleus,  $\begin{array}{c} \text{M} \equiv \quad \text{M} \equiv \\ | \quad \text{or} \quad ||| \\ \text{M} \equiv \quad \text{M} \equiv \end{array}$ .

would be preferable to accept the high valencies of metals suggested. But this is not necessary; and still less necessary is it to return in despair to unitary formulæ. Both extremes seem to involve the idea that the atom of highest valency is a sort of nucleus in the molecule, and enforces an immediate proximity or association of all the other atoms of inferior valency\*. Organic chemistry has clearly shown that this is not true. It is thus most improbable that avoiding the controversial dot (.) in such ways as



and similarly,

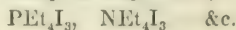
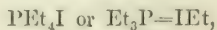
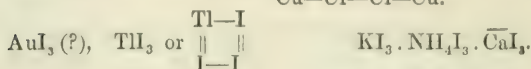
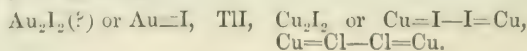


should involve changes in the already high valency of tin or zirconium. Similarly, in  $\text{H}_2\text{PtCl}_6$  it is not very probable that Pt is octovalent; nor in  $\text{K}_2\text{BeF}_4$  that beryllium is hexavalent. The alternative is that the halogens are in some compounds more than monovalent. This view I have taken during the last five years. But we need not, as Wurtz appears to have done, take the higher valency as more than trivalent; for it is not likely that the halogen atom, any more than the metallic atom, is directly associated with all the others. This hypothesis of the occasional trivalency of the halogens (especially iodine, whose evidence is the strongest†), together with the not infrequent tetravalency of oxygen will, I think, be found to simplify our ground very much and practically reduce alternative valencies to two—an alternative which, in the case of such elements as phosphorus and tin, has been recognized for many years.

The elements may, indeed, be arranged in two divisions, viz. those whose valency is a single number unchallenged (*e.g.* K, Na, Ba, Sr, Ca, Mg, Zn, Cd), and those whose valencies are more than one number but probably only

\* To take a metaphor, we are apt to look at the maypole, and the children dancing round it, and to neglect the possible interactions of the children themselves, even granted there is juvenile (chemical) equality among them.

† *E.g.*  $\text{ICl}_3$ ,  $\overline{\text{IAc}_3}$ ,





two, which valence-powers probably all conform to the remarkable law of odd and even numbers, or, as Prof. Odling termed atoms with such valencies, perissads and artiads. If, therefore, I ask that oxygen, chlorine, iodine, and fluorine should be placed in this second class, it is not to complicate but to simplify the great valency problem. Monovalent atoms in such gaseous molecules as  $\text{WCl}_6^*$  and  $\text{PF}_5$  seem highly probable, but they may be questioned in the oxides of chlorine and in the allied oxy-acids. Thus, in the simplest

oxide we really have the choice of  $\begin{array}{c} \text{Cl} \\ | \\ \text{O} \\ | \\ \text{Cl} \end{array}$ ,  $\begin{array}{c} \text{Cl} \\ | \\ \text{O} \\ | \\ \text{Cl} \end{array}$ , and  $\begin{array}{c} \text{Cl} \\ || \\ \text{O} \\ || \\ \text{Cl} \end{array}$ .

Many chemists have accepted  $\begin{array}{c} \text{Cl} \\ | \\ \text{O} \\ | \\ \text{Cl} \end{array}$  and  $\begin{array}{c} \text{K} \\ | \\ \text{O} \\ | \\ \text{Cl} \end{array}$  and  $\begin{array}{c} \text{H} \\ | \\ \text{O} \\ | \\ \text{Cl} \end{array}$

somewhat hastily, because of their historic place in the water-type theory, rather than from positive evidence†.

If the  $\text{KO Cl}$  formula be adopted there is the alternative between  $\text{K—O—Cl}$  and  $\text{K—O}\equiv\text{Cl}$ . But the chemical evidence seems rather in favour of  $(\text{K} \cdot \text{Cl} \cdot \text{O})_x$ , in which case chlorine is trivalent or tervalid as  $\text{K—Cl=O}$ ; or in  $\text{K}_2\text{Cl}_2\text{O}_2$

$\text{K—Cl=O}$   
as  $\begin{array}{c} || \\ \text{K—Cl=O} \end{array}$ . The remarkable increasing chemical stability

of the series  $\text{KClO}$ ,  $\text{KClO}_2$ ,  $\text{KClO}_3$ ,  $\text{KClO}_4$  remains to be explained; but I believe that the hypothesis of the valency variation of one or both the negative elements must be adopted, so long as valency language is useful in focusing mysteries enshrined within a chemical molecule. Possibly the oxygen atoms are in some sort of "closed ring." Certainly the old tandem arrangement  $\text{K—O—O—O—O—Cl}$  must be abandoned. In  $\text{KClO}_3$  and  $\text{KClO}_4$ , the K and Cl may each be separately associated with an oxygen atom; or, seeing that  $\text{KCl}$  is such a constant product whether by wet or dry reactions, there is something to be said for 'Cl''' in some such

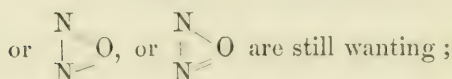
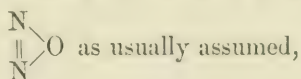
\* Just above the boiling-point.

† Undoubtedly the formula  $\text{Cl(OH)}$ , that is,  $\text{Cl}'\text{—O}''\text{—H}$ , and notably the  $\text{CN(OH)}$  formula for cyanic acid, established themselves under shelter of a somewhat dangerous, or at least premature, extension of the hydrate theory, or  $(\text{OH})$ , so well established in the case of organic acids. The tendency to introduce unwarrantable  $\text{OH}$ -groupings is especially shown in such cases as  $\text{Ag(OH)}$  and even  $\text{NH}_4(\text{OH})$ . The distinctions between hydrates and hydroxides should also be more carefully observed; the former are really quite in a minority. The reaction against the indiscriminate use of  $\text{OH}$ -groupings in inorganic chemistry in many "not proven" cases is now beginning to show itself clearly.

*Phil. Mag.* S. 5. Vol. 25. No. 154. March 1888. R

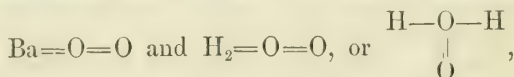
formulae as  $K-Cl=(O_3)''$  and  $K-Cl=(O_4)''$ , where the  $(O_3)$  and  $(O_4)$  are at present indeterminate bivalent radicals.

There is of course no evidence at present available for the quadrivalency of oxygen to be clearly derived from the oxides of nitrogen. The means of determining whether  $N_2O$  is



but when any are given in our text-books the alternatives should surely be given and the whole marked with a conspicuous ?

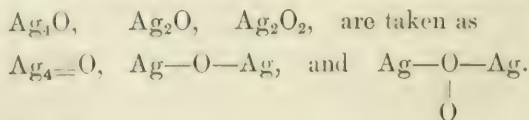
It was in fact in the compounds already referred to,  $H_2O_2$  and  $BaO_2$ , that I at first concluded the tetravalency of oxygen to be the lesser of two puzzles. I look upon them as



allowing of course that neither are formulae of strictly comparable molecules.

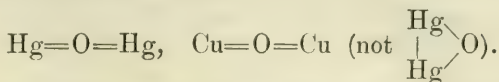
(i.) The peculiar reactions of  $BaO_2$ ,  $MnO_2$ ,  $PbO_2$ , and their inability to form what are curiously called "corresponding salts," as contrasted with  $SnO_2$ ,  $SiO_2$ , are suggested to me by the formulae  $X''=O=O$  as opposed to  $X^{IV}O_2$ , *i. e.*  $O=X=O$ , the molecules being  $(XO_2)_x$  where  $x$  is unknown.

(ii.) The existence of a series of oxides in the case of some metals, and of only one oxide with others, is probably connected with this double valency—I use the phrase provisionally as the sequel will show—of oxygen. Some of the oxides are otherwise not accounted for, and in others there is an assumed linking or double linking of the metallic atoms of which we may well be sceptical. For it is, I believe, the peculiar characteristic of the non-metals or negative elements to preferably exhibit this single, double, or treble linking (if it exists, and whatever it may mean) and thus to accumulate. Thus the oxides of silver,



Similarly,  $Hg$  and  $Cu$  being clearly defined divalent metals,

I prefer to take their suboxides as



There is no clear evidence of the  $\text{Hg}-\text{Hg}$  association.

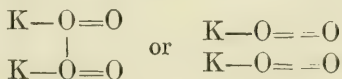
Similarly for  $\text{Cu}_2\text{Cl}_2$  and  $\text{Hg}_2\text{Cl}_2$ , usually assumed as  $\text{Cu}-\text{Cl}$  and  $\text{Hg}-\text{Cl}$ , I should prefer  $\begin{array}{c} \text{Cl}=\text{Hg} \\ | \\ \text{Cl}=\text{Hg} \end{array}$  and  $\begin{array}{c} \text{Cl}=\text{Cu} \\ | \\ \text{Cl}=\text{Cu} \end{array}$  as being more in accordance with the facts of the reactions of these bodies. The cases of the chlorides and double chlorides are very well known.

(iii.) There are certain suboxides which lurk in our text-books and literature. In addition to  $\text{Ag}_4\text{O}^{\text{IV}}$ , perhaps the "blue crust" forming on recently cut potassium is  $\text{K}_4\text{O}^{\text{IV}}$ , and  $\text{Pb}_2\text{O}$  may be  $\text{Pb}=\text{O}=\text{Pb}$  giving  $\text{PbO}$ , which can, like  $\text{BaO}$ , take up more oxygen from the air. Indeed the curious actions of heat on  $\text{PbO}$  and  $\text{BaO}$  as well as  $\text{MnO}_2$  &c. seem to be due to this *conditional* tetravalency of oxygen, and not to change of valency of metallic atom or atoms usually assumed.

(iv.) The peroxides of potassium and sodium are puzzles. As  $\text{Na}_2\text{O}_2$  is not decomposed on heating, it has a legitimate claim

$\begin{array}{c} \text{O}-\text{Na} \\ | \\ \text{O}-\text{Na} \end{array}$  which  $\text{H}_2\text{O}_2$  has not. Moreover it forms a hydrate

$\text{Na}_2\text{O}_2 \cdot 8\text{H}_2\text{O}$ . Whatever  $\text{K}_2\text{O}_4$  may turn out to be, it is probable that all the O atoms are not divalent. The arrangement is undoubtedly a comparatively stable one, and when it is interpreted we shall probably see the constitution of, or at least the interrelations of, certain bodies not usually associated with it, *e. g.*  $\text{K}_2\text{O}_4$ ,  $\text{KClO}_4$ ,  $\text{HClO}_4$ ,  $\text{Cl}_2\text{O}_4$ , all strikingly "stable." Perhaps

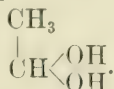


may serve to suggest the problem before us.

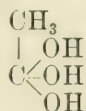
(v.) But just as the remarkable way in which so many metallic chlorides combine with other chlorides, chlorhydric acid, and oxides, is a strong line of evidence for the trivalency of chlorine, so the peculiar actions of water as an active chemical reagent, even when it is not represented in our conventional equations, seem to be strong evidence of the ready way in which the oxygen atom in water can be stimulated, so to speak, to exhibit its tetravalent character.

We may recall the way in which "aldehyd hydrate

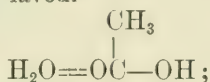
$C_2H_4O \cdot H_2O$ ," for instance, has fought its way out of the ambiguous company of dotted "molecular aggregations" to be viewed as aldehydrol



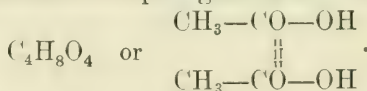
Similarly for acetal and other bodies. Even  $C_2H_4O_2 \cdot H_2O$  has been suggested to be



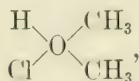
though it is not easy to recognize the chemical equality of the first hydroxyl grouping with the others. There are facts which preferentially favour



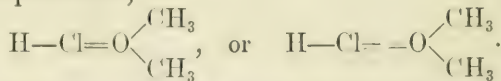
and the recent researches by Ramsay and Young show the existence of the more complex gaseous molecules



(vi.) In the interesting so-called molecular compound discovered by Friedel,  $(CH_3)_2O \cdot HCl$ , a remarkably stable body, we have a most striking case for the tetravalency of oxygen, either as



or, as seems preferable,

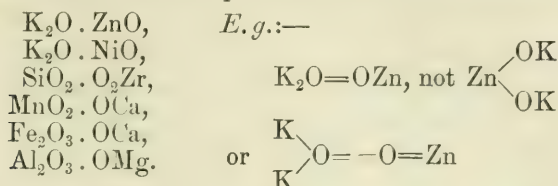


(vii.) This stable compound brings us back to the three types  $HCl$ ,  $H_2O$ ,  $H_3N$  with which we started; but before resuming their consideration, allusion must be made to the polymerization of aldehyds, of cyanates, of the metaphosphates, and other bodies as closely connected with the tetravalency of oxygen\*.

\* The stimulus—if I may so term it—to polymerization in these cases seems to be due to the development, in ways that current research are seeking to explain, of the higher valency, or, as it may be preferably termed, the validity of oxygen, or the tervalidity, for instance, of chlorine in hydro-

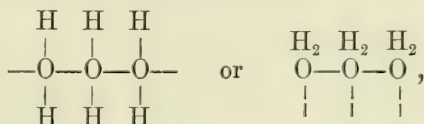


(viii.) Again, the tetravalency of oxygen cannot well be dissociated from such compounds as



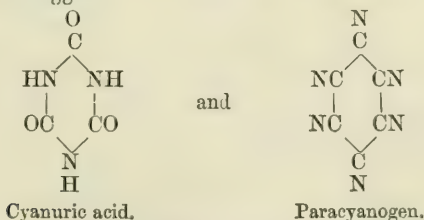
(ix.) The numerous hydroxides, as well as hydrates \*, the so-called "basic salts" so numerous in the case of oxygen, be it noted, and probably also the complex poly-tungstates and silicates are also probably connected with this property of oxygen. Prof. Carnelley's vanadates of thallium form remarkable instances of the convenience of the mode of viewing a salt as a combination of two oxides.

(x.) The water of crystallization of salts may be due to this association of the water molecules



where the oxygen is tetravalent or quadrivalent. The tandem paper arrangement of course suggests nothing of the spatial arrangement, which in all crystals is probably symmetrical. The go-between influence of  $\text{H}_2\text{O}$  in Prof. Dixon's famous

chloric acid solution used as "a polymerizer"  $\text{H}-\text{Cl} \rightleftharpoons$ . It is significant that there are few cases of polymerization where oxygen is not concerned. It does not seem probable, however, that oxygen is tetravalent or quadrivalent in cyanuric acid. The ordinary major valencies and the properties of the substance suggest

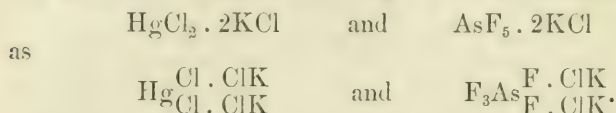


Probably more ring formulæ in inorganic bodies will come to light, and they will affect the maypole hypothesis—as it may be called—of the domination of the central polad atom.

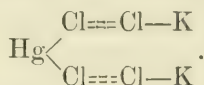
\* Even in the case of so-called "KHO" there is more to be said for  $\text{K}_2\text{H}_2\text{O}_2$ , at least, if not  $(\text{KOH})_x$ .  $\text{Ag}_2\text{O}_2\text{H}_2$ , though doubtful, is less misleading than  $\text{AgOH}$ .

experiments on the explosion of carbonous oxide\* with oxygen is also probably due to this provisional tetravalency of oxygen.

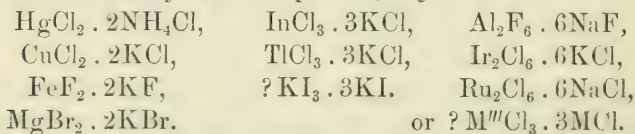
Undoubtedly the chief difficulty which is present to the mind in using these links or conceding these valencies, in the case of proposed higher values, is the doubt whether the combination is of the same order or intensity so to speak, and can be strictly called chemical combination in all cases. On this account Professor Armstrong † wrote the compounds



But the valency as matter of discussion might perhaps be indicated as



There are many such compounds; *e. g.*



These formulæ clearly suggest the problem in quantivalence which lies before us. Certainly such phrases as “residual valency,” “surplus affinity,” “residual charge on the atoms” are open to the objections which have already been suggested. They may prematurely beg the problems of chemical affinity. Even in the case of  $\text{PCl}_5$  there can scarcely be said to be observed a perfect chemical equality of the atoms such as we believe to be in  $\text{CH}_4$ . The chlorine is apparently dealt with preferably in two fifths and three fifths in most chemical reactions. Such considerations have long induced Professor Kekulé and others to maintain that a phosphorus atom is constantly trivalent.

For the higher valency under discussion, in the case of oxygen and chlorine, if we are to use other words, it may be found convenient to use a kindred word *validity*, which has therefore been occasionally used in this paper. We

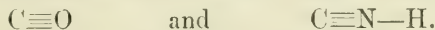
\* Water-molecules necessarily present would be formulated  $\begin{array}{c} \text{H} \\ \diagup \quad \diagdown \\ \text{O} = \end{array}$   
*Cf.* note on the last page.

† B. A. Report, Aberdeen, 1885.

could say, for instance, that chlorine is usually monovalent but occasionally tervalid. The latin prefixes, roman numbers, and broken lines in formulæ would help to keep up the distinction ; thus :—

—R'	monovalent,	---R <sup>I</sup>	univald.
=R''	divalent,	===R <sup>II</sup>	bivald.
≡R'''	trivalent,	===R <sup>III</sup>	tervald.
≡R''''	tetravalent,	===R <sup>IV</sup>	quadrivald, &c.

We might then say that carbon is tetravalent, except in the gaseous molecules CO and CNH, where it is bivald unless the tetravalency of oxygen and the pentavalency of nitrogen are admitted, when carbon is always tetravalent\*.



Other open questions will suggest themselves to the mind†.

The general chemical observation that no atom is really known to be more than hexavalent to the standard monovalent atoms‡ finds remarkable independent support in Prof. J. J. Thomson's development of the vortex-atom theory. According to his view a triad atom consists of three vortex rings symmetrically linked together, and "no element could have an atom consisting of more than six vortex rings." He has also found that stable motion exists "when the distance between the rings was small compared with their apertures," and he adds: "We found that for each displacement there are two periods of vibration, a quick vibration and a slow one." Is it possible, it may be asked, that in these two periods of vibration there is a connexion with the two valencies or rather the valency and validity of so many of the negative or non-metallic elements which have been referred to in this paper?

Prof. J. J. Thomson at least clearly points out that, according to the vortex-atom theory of gases, "one atom of a

\* In CO, according to Prof. J. J. Thomson's conception of links, "the atom of carbon has the same number of links as the atom of oxygen."

† CNH and NCH are metameric molecules. It is not easy to decide which is the formula of prussic "acid" gas. In the formula, I have assumed it to be the "imido" relative of CO, which is metameric with

formo-nitrile,  $\begin{array}{c} \text{H} \\ | \\ \text{---C---N} \\ | \\ \text{CH} \end{array}$ . The two methyl and ethyl metamers seem distin-

guishable, but sometimes turn up together in "the same" reaction.

‡ The cases of M<sup>VI</sup> and M<sup>VIII</sup> are here considered as not proven. Prof. J. J. Thomson's views on this point will doubtless be dealt with in the course of further chemical research.

dyad might unite not only with two atoms of a monad, but also with four atoms of a monad, the four atoms of the monad splitting into two groups each." He says: "Regarding oxygen, then, as a dyad, the molecule of water consists of three primaries  $H_2-O-O$ ." I do not know the exact significance of his marks, but the analogous formula now proposed is  $H_2=O=O$ , or at least  $H_2=O==O$ . It is at least

O—H  
|  
O—H

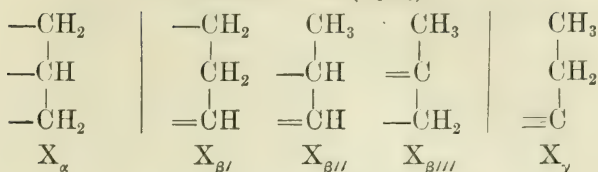
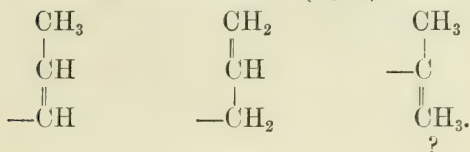
preferable to the symmetrical in better exhibiting its

reactions, and as the modern representative of the late Sir B.

Brodie's  $H_2\overset{+}{O}\overset{-}{O}$ , which has the no small merit of distinguishing between the two atoms chemically dissimilar. The novelty (so far as I know) of formulating a divalent and a tetravalent atom of the same element in one chemical molecule will, I hope, provide fruitful discussion with those chemists who are not prepared to reject or scorn the child-hood of constitutional formulæ.

However hypothetical our constitutional formulæ of most inorganic bodies may yet be, I venture to doubt the wisdom of abandoning the use of our valence-marks, linkings, so-called double bonds, &c. in favour even of the single mark proposed some years ago by Lössen. They may still be used with advantage to chemical science, but not usually to those who are beginning its study. The fact is that the one-sided study of inorganic chemistry, with its numerous elements, and therefore greater delicacy and intricacy of valency-variation, would make us pessimists, and the isolated study of organic chemistry, with its few elements, would keep us optimists. The reaction of the constitutional school of chemists upon the empirical formulæ of inorganic chemistry has been, in some respects, rather premature in its attempts to solve too much at the outset; but the real laws of valency must be substantially the same in both. Thus we have the radical or grouping—the organic atom so to speak—of ethyl ( $C_2H_5$ ) always of one valency, but ( $C_3H_5$ ) is both mono- and tri-valent. Nay, more. What would be called the allotropic modifications of this "atom"—were it an atom—are well known, and can be separably recognized. If ( $C_3H_5$ ) were an elementary atom, it would be called usually trivalent but occasionally univalent; or, if no distinction is to be made, trivalent and monovalent after the fashion of gold or thallium. There are not less than five trivalent ( $C_3H_5$ )'; and their compounds with either chlorine, oxygen, or hydroxyl are all known.



Trivalent  $X''' = (C_3H_5)'''$ .Monovalent  $X' = (C_3H_5)'$ .

If at present we cannot press the analogy too close in the alternating valency\* of so-called elementary radicals or atoms, especially the non-metallic ones, we are surely not yet prepared to abandon formulæ which have been of such signal service in the development of organic chemistry, and to abolish the notation because we do not yet know the real meaning either of the marks or valency. But is it too bold a thing for us to look forward to the time when we can similarly deal with an elementary radical, the present provisional chemical atom? At present our notions of valency are chiefly conditioned by the experiments made, *and the chemical products constantly being beheld*, at about 16° C. and 76-centim. mercury pressure. It is, therefore, wonderful and encouraging that we should already have a theory which, on the whole, has such promise for the future. And it is not surprising that, as Clerk Maxwell wittily put it at Belfast,

Though they seem to cling together,  
And form "associations" here,  
Yet soon or late they burst their tether,  
And through the depths of space career.

Oxford, 7th November, 1887.

\* This theory of an *alternating* valency, as contrasted with both the "almost any" and the "constant one" theories, not only appears to have some support from the vortex-atom theory but from the periodic law, especially when this is exhibited on Prof. J. Emerson Reynold's plan, as exposed by Dr. Crookes. But I see no sufficient reasons for the septavalencies.

XXVIII. *Proceedings of Learned Societies.*

## GEOLOGICAL SOCIETY.

[Continued from p. 74.]

Dec. 21, 1887.—Prof. J. W. Judd, F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Correlation of some of the Eocene Strata in the Tertiary Basins of England, Belgium, and the North of France." By Prof. Joseph Prestwich, M.A., F.R.S., F.G.S.

Although the relations of the several series have been for the most part established, there are still differences of opinion as to the exact relation of the Sable de Bracheux and of the Soissonais to the English series; of the Oldhaven Beds to the Woolwich series; and of the London Clay and Lower and Upper Bagshots to equivalent strata in the Paris basin. The author referred to the usual classification of the Eocene Series, and proceeded to deal with each group in ascending order.

The *Calcaire de Mons* is not represented in England, but may be in France by the Strontianiferous marls of Meudon. It contains a rich molluscan fauna, including 300 species of Gasteropods, many of which are peculiar, but all the genera are Tertiary forms. The *Heersian* are beds of local occurrence, and the author sees no good reason for separating them from the *Lower Landenian* or *Thanet Sands*. He gave reasons for excluding the *Sands of Bracheux* from this group. Out of 28 Pegwell-bay species, 10 are common to the Lower Landenian, and 5 to the Bracheux Sands, which present a marked analogy with the Woolwich Series. These Sands of Bracheux are replaced in the neighbourhood of Paris by red and mottled clays. Out of 45 species at Beauvais only 6 are common to the Thanet Sands and 10 to the Woolwich Series. Out of 75 species in the *Woolwich and Reading Beds* 19 occur in the Bracheux Beds, if we add to these latter the Sands of Chalons-sur-Vesles.

Respecting the *Basement Bed* of the London Clay (*Oldhaven Beds* in part), the author would exclude the Sundridge and Charlton fossils, which should be placed on a level with the Upper Marine Beds of Woolwich. He allowed that the former were deposited on an eroded surface, but this involves no real unconformity, whilst the palaeontological evidence is in favour of this view, since out of 57 species in the Sundridge and associated beds, only 16 are common to the London Clay. He therefore objected to the quadruple division. Either the Oldhaven should go with the Woolwich or with the Basement Bed. He admitted that the term "Basement Bed" is objectionable, and preferred Mr. Whitaker's term for the series, as he would limit it.

The *Lower Bagshot Sands* the author would call "London Sands" whose Belgian equivalent is the Upper Ypresian, and the French the Sands of Cuise-de-la-Motte, forming the uppermost series of the Lower Eocene. A group of fossils has been discovered in the

Upper Ypresian sands of Belgium, which leaves no doubt of their being of Lower Eocene age, and consequently the Lower Bagshots must be placed upon the same horizon. There is no separating line of erosion between the London Clay and the Lower Bagshots, the upper part of the former is sandy, and the lower part of the latter frequently argillaceous. Similarly no definite line can be drawn between the Upper and Lower Ypresian; but in both countries this series is separated from overlying beds by a well-marked line of erosion. So also in France the base of the *Calcaire Grossier* (Bracklesham Beds) is a pebbly greensand resting on an eroded surface of the Sands of the Cuise-de-la-Motte. In Belgium, in Whitecliff Bay, and in the Bagshot district the Upper Eocene rests upon an eroded surface of the Lower Eocene. Subjoined is the author's proposed classification of the Eocene:—

*Proposed Classification of the Eocene.*

	ENGLAND.	BELGIUM.	FRANCE (Paris Basin).
UPPER ... {	<i>a.</i> Barton Beds.	<i>a.</i> Wemmelian.	<i>a.</i> Sables Moyens or Grès de Beauchamp.
	<i>b.</i> Bracklesham Beds. = Upper and	Laekienian and	<i>b.</i> Upper Calcaire Grossier.
	<i>b*</i> . Middle Bagshots.	Bruxellian.	<i>b*</i> . Glauconie Grossière.
LOWER ... {	Wanting.	Paniselian.	{ Sands of Cuise-de-la-Motte.
	London Sands = Lower Bagshot.	Upper Ypresian.	
	London Clay.	Lower Ypresian.	Wanting.
	Basement or Oldhaven Beds.	?	{ Sables Inférieurs of the Soissonnais, including the Marls and Sands of Billy, the 'Lignites' and Sands of Bracheux.
	Woolwich and Reading Beds.	{ Upper Landenian.	
	Thanet Sands.	{ Lower Landenian and Heersian.	Sands of St. Omer, Douai, and La Fère.
	Wanting.	Calcaire de Mons.	Strontianiferous Marl of Meudon?

2. "On the Cambrian and Associated Rocks in North-west Cacr-narvonshire." By Prof. J. F. Blake, M.A., F.G.S.

After referring to the published views of Professor Sedgwick, Sir A. C. Ramsay, and the Geological Survey, Profs. Hughes and

Bonney and Dr. Hicks concerning the area in question and especially as to the presence or absence of Precambrian rocks, the author gave an account of his own explorations and their results, the principal of which were the following.

In the Bangor and Caernarvon area three distinct conglomerates had been confounded. The only one that showed distinct unconformity on the underlying rock was of Arenig (Ordovician) age. The rocks of the southern and central portion of the area were essentially of igneous origin and might be distinguished into two groups, the southern probably intrusive, the northern certainly eruptive. There is no evidence to show what interval of time elapsed between the production of these two groups, nor which of them is the earlier, although the author regards it as more probable that the southern mass is of the earlier date and overlain by the northern portion. The Bangor beds are derived from the denudation of the volcanic series, and of rocks which may have been associated with it, and they contain a series of conformable conglomerates of which the great conglomerates near Bangor are members. They are the continuation of the Cambrian rocks seen to the east, and have not undergone any serious alteration. The porphyries of Llyn Padarn and Moel Tryfaen are contemporaneous lava-flows in the midst of the Cambrian series, the overlying conglomerates being derived from them and from the sedimentary Cambrian rocks to the west; and hence there is no certain proof of there being any Precambrian rocks in the whole district, though it is probable that the rock near Caernarvon belongs to an epoch distinct from and anterior to the Cambrian.

Jan. 11, 1888.—Prof. J. W. Judd, F.R.S., President, in the Chair.

The following communications were read :—

1. "On the Law that governs the Action of Flowing Streams."  
By R. D. Oldham, Esq., F.G.S.

The author, after describing how his attention was drawn to the subject, proceeded to an investigation of the law that governs the action of a flowing stream. Having accepted as a fundamental principle that the velocity of a stream will always tend to become such as is just sufficient to transport the solid burden cast on to the stream, and pointed out that the principle is almost axiomatic in its nature, he finds that, where untrammelled by exterior conditions, a stream will be alternately confined to a single, well-defined, deep channel, and spread out into a number of ill-defined, shallow channels, the former being defined as a "reach," the latter as a "fan," that the gradient in the "reach" is less than on the "fan," and that both "reach" and "fan" will continually be encroaching at their upper ends, and being encroached upon at their lower ends.

After detailing some general considerations which show that what should occur according to hypothesis, does actually occur in nature, he indicated that the accurate and detailed levels taken in connexion with the Ganges canal, do actually show this alternation of "reach" and "fan," that the gradients are higher in the latter,



as they should be, and that the records of the canal show the retrogression of "fan" and "reach" demanded by the hypothesis.

Accepting this agreement of fact with hypothesis as proof of the correctness of the latter, it follows that the fundamental principle on which it is founded is correct, and that, in the absence of interfering causes of greater potency, it is the coarseness or fineness of the débris cast upon a stream that will determine its gradient and velocity, and not, as stated in text-books, the velocity of a stream that will determine its gradient and the coarseness of the débris transported by it:—a conclusion that might be arrived at independently, from the fact that it is in the upper reaches of a stream, where coarse débris prevails, that high velocities of current prevail, while in the lower reaches, where the débris is finer in grain, the velocity of current is also diminished.

2. "Supplementary Notes on the Stratigraphy of the Bagshot Beds of the London Basin." By the Rev. A. Irving, B.Sc., B.A., F.G.S.

This paper contained the results of field-work during the year 1887. Additional notes on the stratigraphy of the Bracknell and Ascot Hills were given, justifying the reading of the country as shown in figs. 1 and 2 of the author's last paper (Q. J. G. S. August 1887), the examination of this line of country having been extended as far as Englefield Green. Sections of the beds of the Middle Group as they crop out at Cæsar's Camp, Swinley Park, Ascot, and Sunningdale, were described and correlated with the 76 feet of beds which constitute that group in the Well-section at Wellington College.

The stratigraphy of the hills known as Finchampstead Ridges has been worked out from numerous sections on their flanks; and the strata of the Bearwood Hills were correlated directly with them.

All along the northern margin a general attenuation of (a) the Lower (fluvialite) Sands, and of (b) the Middle (green earthy) Sands was shown to occur, and in some places on the northern margin they are found to have entirely thinned away, admitting of distinct overlap at more than one horizon.

The second part of the paper dealt with the Highclere district, where the author believes he has established the full succession of the three stages of the Bagshot Formation, a section being given across the valley south of Highclere Station, showing the succession of the whole Eocene series (with the *Ostrea bellovacina*-bed for its base) as it is developed there.

Some important conclusions were drawn as to the Tertiary physiography of the South of England; and the revised tabulation of the Tertiaries put forward by Prof. Prestwich at the Society's last meeting was referred to as supporting some of the main points for which the author has contended.

3. "The Red-Rock Series of the Devon Coast Section." By the Rev. A. Irving, B.Sc., B.A., F.G.S.

From a recent examination of this section, and from the facts

furnished by Mr. Ussher's paper (Q. J. G. S. vol. xxxii. pp. 367 *et seq.*), the author has arrived at the conclusion that the series of red rocks between the Lias to the east of Seaton and the Carboniferous of Devon, formerly described under the title of "New Red Sandstone," cover the period of geologic time which that term signified, and that the lower members of the series belong, not to the Trias, but to the Permian or Post-Carboniferous.

He considered that at the base of the Budleigh-Salterton Pebble-bed there is a physical break of as much significance as that between the Trias and the Permian of the Midlands. From this point eastwards the Triassic system is represented by a series of rocks quite comparable with the Bunter and Keuper of the Midlands, the Bunter being here represented by the Middle Division (about 200 feet thick) and the Upper Division of Prof. Hull.

These pass under the basement sandstone-series of the Keuper below High Peak and Peak Hills, are brought up again by faulting at Sidmouth, and dip beneath the Keuper again east of the Sid, from which point eastwards the whole Keuper Division is exposed, with quite a normal facies, as seen in the Midlands, in Central Germany (Thüringen, Jena), and in the Neckar Valley.

In the marls which underlie the Budleigh-Salterton Pebble-bed, he recognized the equivalents of the Permian Marls of Warwickshire and Nottinghamshire, and of the Zechstein Marls of Germany. These pass, by a gradual transition, through Sandstones, becoming more and more brecciated, into the great brecciated series of Dawlish and Teignmouth, which were regarded as the equivalents of the great Permian breccias of the west of England, of Ireland, and of the Lower Rothliegendes of Germany.

All the rocks below the Budleigh-Salterton Pebble-bed were regarded as the assorted materials furnished by the detritus of the palæozoic mountain-region of Devon, Cornwall, and Brittany, and as representing the waste and degradation of that region, deposited on the mountain-flanks and in land-locked bays during Post-Carboniferous times, the marls being compared with the Nyírok of the Austrian geologists.

## XXIX. *Intelligence and Miscellaneous Articles.*

### AN EXPERIMENT ON THE EMISSION OF LIGHT BY IGNITED BODIES. BY FERDINAND BRAUN.

A SMALL portion, a few square centimetres for instance, of any object of porcelain is coated with the ordinary black paint used by painters on porcelain\*, and is heated in a muffle closed

\* This is a mixture of several metallic oxides with a flux; that is, an easily fusible silicate or borate. It is rubbed to a soft paste with some fresh oil of turpentine to which a little "thickening-oil" is added, and is painted on with a brush. "Thickening-oil" is the name given by porcelain-painters to the liquid which gradually creeps over the edge of a vessel in which turpentine is exposed in the open air—it is probably a hydrate of turpentine.

on all sides except through a small peep-hole; the following is then observed. At the first beginning of the red heat the porcelain begins to be luminous. The black spot stands out from it. As the temperature rises the emission of light by the porcelain becomes more intense, and the whole contents of the muffle seems as if it had been feebly illuminated from the outside. If the heat be still more increased the black spot becomes feebler, and after passing through a relatively small interval of temperature, it stands out so little from the porcelain that anyone seeing it for the first time would think it had completely disappeared. When a burning spill or a gas-flame is brought into the muffle, it is seen that the spot is still black on a white ground (the black having a dark red rusty tinge). This phenomenon, which might be used for recognizing distinct temperatures, I estimate to occur at  $800^{\circ}$  C. If the temperature be now raised, the emission of light of the dark spot precedes that of the porcelain, and at about  $1000^{\circ}$  to  $1100^{\circ}$  C. it appears bright, radiating white on the rose-red porcelain. By introducing a burning body into the muffle it again appears dark on white. Other porcelain colours, purple for instance, give similar phenomena; a pen-stroke (which changes into oxide of iron and burns in with lustre) is sufficient; but no material gives so intense a colour, and is so little interfered with by reflexion, as that mentioned.

The phenomenon is simply explained. Porcelain, at ordinary and even higher temperatures, is transparent for luminous rays; the dark mixture of metallic oxides, however, is opaque, as can easily be seen with a painted porcelain crucible made red hot in the gas-flame. In the measure in which the luminous rays increase in intensity with increase of temperature, the emission of light by the black spot also increases. As it always appears black when illuminated with a source of high temperature, it follows that it must have a greater power of absorption than porcelain; that is, if the heat were increased its brightness would also increase, provided there were not otherwise any change in the power of absorption.

The experiment may of course also be made in a darkened room with a piece of porcelain, and forms thus an instructive lecture-experiment. I find that it succeeds pretty well if a large porcelain crucible is painted on the inside and heated in a Bunsen's burner. The disappearance of the black, however, does not succeed very well; and by free heating in the flame we cannot get a temperature of more than a dark redness. On the other hand, with a spot of gold an intensely green radiation is obtained at about  $800^{\circ}$  (like the surface of melted copper), which at decreasing temperature passes into a dark blue. The colours recall the colours of transmission of thin gold leaves. Platinum is far more luminous on cooling than porcelain; the light disappears, passing through a faint red, like that of the other opaque solids. The behaviour of gold and platinum shows distinctly that certain kinds of rays have a *specific* emission.—Wiedemann's *Annalen*, No. 2, 1888.



ON CAPILLARY ANALYSIS : A METHOD OF SEPARATING AND  
RECOGNIZING INDIVIDUAL COLOURING-MATTERS IN THEIR  
MIXTURES. BY F. GOPPELSRODER.

The properties and reactions of the pure colouring-matters are accurately studied, and therefore the nature of a particular colouring-matter may be pretty readily ascertained. But the separation of the constituents of colouring-matters is in general a very difficult problem. The new method of capillary analysis renders it possible in many cases to form in a short time a judgment as to the composition of even very complicated substances. The basis of the method is very simple, and is already contained in the researches of Schönbein. If a strip of white unglazed paper is placed in any given solution, the solvent rises more or less rapidly than the dissolved body. Different dissolved substances have different rates of travelling.

If the bottom-end of a strip of pure filtering paper is dipped in a not too concentrated solution of the mixture to be investigated, according to circumstances in from 15 minutes to 12 hours, a series of zones of different colours are obtained, which contain the individual colouring-matters. The separate zones are noted as to their colour and breadth, and are then treated with suitable solvents. With the extracts thus produced fresh capillary experiments are made, and the entire operation is repeated until zones of the individual pure colouring-matters are obtained, which can be examined according to the ordinary reaction-methods.

The new method has done the author good service in examining food, and various other articles as to the addition of colours : for instance, of picric acid in beer, fuchsine in wine &c.—*Beiblätter der Physik*, vol. xi. p. 754 (from *Romen's Journal*, No. 1, 1887).

---

ON THE CONVECTION OF ELECTRICITY BY EVAPORATION.

BY DR. ERNST LECHER.

The author first of all shows, by thermometric means, that an electrified liquid evaporates more rapidly than an unelectrified one under the same circumstances. It cannot, however, be concluded that this is a consequence of the electrification of vapour ; it is primarily and almost exclusively a consequence of the electrical aura, which also renders impossible any decision as to the far more subtle question of the magnitude of the heat of evaporation of an electrified liquid.

It is moreover proved by a simple arrangement of the experiment that, by strong electrification of a liquid surface, an electrified cloud (or a cloud of electrified drops) may be formed ; that is, its inductive action can be demonstrated.—*Sitzungsberichte der kaiserlichen Akademie in Wien*, June 1887.



THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

---

[FIFTH SERIES.]

---

APRIL 1888.

---

XXX. *The Absolute Wave-length of Light.* By LOUIS BELL,  
*Fellow in Physics in Johns Hopkins University\*.*

THIS paper contains the final results of the research partially reported in this Journal for March 1887. In view of the wide discrepancies in the value of this physical constant as determined by various observers and methods, it has seemed desirable to give in brief the history of the subject, and to discuss critically certain portions of the investigation which have proved stumbling-blocks in the past. I refer particularly to the verification of the standards of length employed, and to those errors of ruling in the gratings which may, and usually do, produce errors in the result obtained.

The first portion of this paper will be devoted to the methods and results of the pioneers in this work, and the methods, apparatus, and standards of length employed in the present investigation.

The second portion will contain the details of the experimental work, together with a discussion of the final results and those questions of theoretical and practical interest which arise in connexion with the work of recent experimenters. With this preliminary notice is presented the first half of the paper.

*Historical.*

Fraunhofer's first paper on the lines which bear his name marks a new era in the science of optics. Up to that point any careful study of spectra had been impossible for lack of

\* Communicated by the Author.

definite standards of reference, and because the apparatus was as yet very defective. Fraunhofer's research, "*Bestimmung des Brechungs- und Farben-zerstreuungs-Vermögens verschiedener Glasarten*," was presented to the Munich Academy of Sciences in 1814, and was published in the fifth volume of the *Denkschriften*. It then became possible to study in detail the properties of rays of definite position, and the work was taken up almost immediately. Almost the first step was to determine the wave-lengths of prominent points in the solar spectrum; and, as is well known, Fraunhofer himself took it, determining the wave-lengths corresponding to his lines B, C, D, E, F, G, H. As there seems to have been (noticeably in Verdet's papers) some confusion concerning his papers on this subject, it may be well here to clear the matter up.

Fraunhofer's first paper dealing with the subject was presented to the Munich Academy in 1821. It is entitled, "*Neue Modifikation des Lichtes durch gegenseitige Einwirkung und Beugung der Strahlen, und Gesetze derselben*," and was printed in the eighth volume of the *Denkschriften*. It is of considerable length, and deals with various diffraction phenomena, but its chief interest lies in the wave-length measurements made with wire gratings. The experiments made with ten of these are given in detail, and are remarkably careful and consistent. The gratings were quite various, the wires being from 0.04 to 0.6 millim. in thickness, and the grating-space, as ordinarily measured, from 0.0528 to 0.6866 millim. From these proportions it is evident enough that the spectra must have been imperfect; but, in spite of this, Fraunhofer obtained results which agreed remarkably well with each other. The wave-lengths of D, as obtained from the above-mentioned ten gratings, were as follows, reduced to millimetres:—

	millim.		millim.
(1)	0.0005891	(6)	0.0005888
(2)	0.0005894	(7)	0.0005885
(3)	0.0005891	(8)	0.0005885
(4)	0.0005897	(9)	0.0005882
(5)	0.0005885	(10)	0.0005882

The mean value adopted was 0.0005888 millim., which, considering the gratings, and the fact that most of the angles of deviation were less than  $1^\circ$ , is certainly remarkably accurate. It should be noted, too, that the finer gratings (1) to (4) gave even better results.

A brief discussion of this paper appeared in the seventy-

third volume of Gilbert's *Annalen*, and a French reprint in Schumacher's *Astronomische Abhandlungen* (ii. p. 46).

Fraunhofer's second and more complete paper appeared in 1823 in Gilbert's *Annalen* (lxxiv. p. 337). Its title is, "Kurzer Bericht von den Resultaten neuerer Versuche über die Gesetze des Lichts, und die Theorie derselben." This paper gives a detailed account of his experiments with two glass gratings. Of these, the grating-spaces were respectively 0.0033 and 0.0160 millim. The former was apparently much the better, and upon it Fraunhofer based his final result, which for D was 0.0005886 millim., while the experiments with the coarser grating gave 0.0005890 millim. These values apply quite certainly to the mean of the two D lines, and not, as has been sometimes supposed, to one of them alone.

The experimental work with these glass gratings was much better than with the previous wire ones, since the angular deflections were very much larger, and the gratings themselves were susceptible of far more exact measurement. But at best they were but indifferent instruments, and the terminal lines were so bad that they had to be retraced before the grating-space could be determined. So, between poor gratings and indifferent standards of length, Fraunhofer's determination of absolute wave-length left very much to be desired. However, nothing much better could be accomplished until the art of making gratings was very much improved; and it was not until Nobert's gratings became tolerably well known that any serious attempt was made to improve on Fraunhofer's results. From time to time various investigators worked at the problem, both with Nobert's earlier gratings and by utilizing various interference phenomena. When, however, the great investigations of Bunsen and Kirchhoff revolutionized spectroscopic work and emphasized its great importance, the attention of scientific men was called to the need for accurate measurements; and for half a dozen years investigators were active, and Mascart, Ditscheiner, and Ångström appeared on the field almost simultaneously. Each published a paper in 1864, and of these that of Mascart is probably the most accurate and painstaking, though now it is quite certain that the values he obtained were considerably too small. He employed four or five of Nobert's gratings, and instead of placing the grating perpendicular to either the collimator or the observing-telescope, used it in the position of minimum deviation, that is to say, so that the plane of the grating should bisect the angle formed by the incident and diffracted rays. This position has certain advantages; but as the experimentation is rather more difficult than in the ordinary position, the

method appears to be of somewhat questionable utility. It avoids, to be sure, the necessity of placing the grating normal to the axis of either telescope; but as there is very little trouble in making this adjustment with a high degree of accuracy, and keeping it through a series of measurements, the gain is by no means considerable. Aside from this question, Mascart's spectrometer read only to five seconds; and while his results with different gratings agree very well individually, they are certainly collectively in error by quite a large amount, very possibly owing to bad standards of length.

It is a fact to be noted in discussing all these earlier wavelength determinations, that sufficient attention was not paid to the measurement and study of the gratings—by all odds the most difficult part of the problem. The angular measurements of any one of the above investigators were good enough to have given very exact results had they been combined with proper investigations of the grating-spaces. As most of Nobert's gratings were small and by no means accurately ruled, there was peculiar need of care in measuring them; and when one considers that the defining-lines on most standards of length are far from being good, it is clear that the chances of error were numerous. In Ångström's first paper he even relied on the grating-space assigned by the maker. Ditscheiner employed a grating which had belonged to Fraunhofer himself; but the number of spaces was uncertain, and this led to a large error, which he corrected, in part, in a supplementary paper some years later. Ditscheiner's principal paper was published in 1866; and was followed in 1868 by an elaborate discussion of the whole problem by Van der Willigen, whose paper is valuable mainly for a particularly elaborate review of sources of error. Like his predecessors, he used Nobert's gratings; but as the construction of his spectrometer confined his angular measurements to the deviation on one side of the normal, their accuracy may be somewhat open to question; while his standard of length was anything but reliable, as it was a glass scale only three centimetres long, and the only assurance of its accuracy was the certificate of the maker that it was "*très exacte*" at 50° Centigrade. For one or both of the above reasons, Van der Willigen's results were larger than any which have been obtained, before or since his time.

In the same year appeared Ångström's great research which has so long served as the standard in all questions of wavelength. It is hard to say too much of the conscientious and painstaking experiments on which his results were based; and any want of accuracy in the final result was due to no lack of skill or care on his part, but rather to the imperfect instruments with which he was obliged to work. Like every one



before him, he used Nobert's gratings; and in spite of the fact that, like all Nobert's gratings, they gave very imperfect definition and showed numerous "ghosts," his results were more than usually consistent. But, in spite of all Ångström's care, the event has shown that his wave-lengths are in error by as much as one part in seven or eight thousand, mainly through an error in the assumed values of his standards of length. Ångström measured his gratings by means of a dividing-engine, the screw of which was very exactly determined by comparisons resting on the Upsala metre, which, in turn, had been compared by M. Tresca with the prototype of the Conservatoire des arts et métiers. Had this comparison given the correct value of the Upsala metre, Ångström's wave-lengths would have been very nearly exact except for corrections due to errors of ruling in the gratings.

After Ångström's research the question of absolute wave-length was not seriously raised for ten years; when Mr. C. S. Peirce, under the auspices of the United States Coast Survey, again attacked the problem, armed with Rutherford gratings far superior to those used in any previous research. No official report of his very elaborate and exhaustive experiments has ever been published, save a very brief preliminary report in the *American Journal of Science* in 1879. Such of his results as have been made in any way public will be discussed in the experimental part of the present paper.

Meanwhile Thalén, who so efficiently aided Ångström in his work, had taken up the part of it left uncompleted by the latter's death; and in his paper, *Sur le Spectre du Fer*, published at Upsala in 1885, discussed the corrections which must be applied to Ångström's values by reason of the error in the Upsala metre. It seems that, through the experiments of Professor Lindhagen, Ångström became aware, as early as 1872, that the assumed value of his standard was considerably too small. His death prevented his verification of M. Lindhagen's results, and nothing further was done till Thalén took up the work. Tresca's comparisons had obtained for the length of the Upsala metre at  $0^{\circ} 999.81$  millim. But the very exact experiments of M. Lindhagen have shown the above to be somewhat too small, and that the correct value is  $999.94$ . This difference makes, of course, a marked error in the wave-lengths based on Tresca's results. Applying the appropriate correction, the wave-length of E, the line most carefully determined by Ångström, becomes

5269.80,

instead of the original

5269.12.

This final result of Ångström is certainly entitled to considerable respect, and seems to be subject only to those corrections which may be due to irregularities in the gratings. These were, however, so poor compared with the gratings of today that such corrections would necessarily be of uncertain magnitude.

At all events it is quite sure that, of the wave-length determinations made up to 1880, those of Peirce, and Ångström corrected by Thalén, are by all odds the best. Of the two Peirce's is probably the better, by reason of better gratings; but his work will be discussed in another part of this paper in connexion with the very recent works of Müller and Kempf and Kurlbaum, which merit more extended study than would be in place at this point.

A tolerably complete bibliography of the subject up to date is annexed. Many of the papers are of little except historical value; but they will at least exhibit the various methods employed, and the growth of exact experimentation.

1821. Fraunhofer.—*Denkschr. d. Akad. d. Wiss. zu München*, viii. Heft ii. p. 38. "Neue Modifikation des Lichtes durch gegenseitige Einwirkung und Beugung der Strahlen, und Gesetze derselben."
1823. Fraunhofer.—Schumacher's *Astronomische Abhandlungen*, ii. p. 46.
1823. Fraunhofer.—Gilbert's *Annalen*, lxxiv. p. 337. "Kurzer Bericht von den Resultaten neuerer Versuche über die Gesetze des Lichtes, und die Theorie derselben."
1835. Schwed.—*Die Beugungerscheinungen*. (Mannheim.)
1849. Stokes.—*Athenæum*, No. 1143; *Inst.* xvii. p. 368. "On the Determination of the Wave-length corresponding with any Point of the Spectrum."
1851. Nobert.—*Proc. Roy. Soc.* vi. p. 43; *Phil. Mag.* [4] i. p. 570. "Description and Purpose of the Glass Plate which bears the Inscription—Longitudo et celeritas undularum lucis cum in aere tum in vitro."
1851. Nobert.—*Pogg. Ann.* lxxxv. p. 83. "Ueber eine Glasplatte mit Theilungen zur Bestimmung der Wellenlänge und relativen Geschwindigkeit des Lichts in der Luft und im Glase."
1852. Drobisch.—*Pogg. Ann.* lxxxviii. p. 519. "Ueber die Wellenlänge und Oscillationszahlen der farbigen Strahlen im Spectrum."
1853. Esselbach.—*Berlin Monatsber.* p. 757. "Ueber die Messung der Wellenlänge des ultravioletten Lichts."
1856. Esselbach.—*Pogg. Ann.* xxviii. p. 513; *Ann. de Chim. et de Phys.* [3] 50. p. 121. "Eine Wellenmessung im Spectrum jenseits des Violets."
1856. Eisenlohr.—*Pogg. Ann.* xxviii. p. 353, xcix. p. 159; *Ann. de Chim. et de Phys.* [3] xlix. p. 504. "Die brechbarsten oder unsichtbaren Lichtstrahlen in Beugungsspectrum und ihre Wellenlänge."
1863. Müller.—*Pogg. Ann.* cxviii. p. 641. "Bestimmung der Wellenlänge einiger heller Spectrallinien."
1863. Mascart.—*C. R.* lvi. p. 138. "Détermination de longueur d'onde de la raie A."
1864. Mascart.—*C. R.* lviii. p. 1111. "Détermination des longueurs d'onde des rayons lumineux et des rayons ultraviolets."

1864. Mascart.—*Ann. de l'Ecole Normale*, i. p. 219. "Recherches sur la détermination des longueurs d'onde."
1864. Bernard.—*Mondes*, v. p. 181. "Théorie des bandes d'interférence . . . . . Longueur d'onde de la raie A." . . . .
1864. Stefan.—*Ber. d. Wien. Acad.* 50. Heft ii. p. 31; *Pogg. Ann.* cxxii. p. 631. "Ueber die Dispersion des Lichtes durch Drehung der Polarizations-ebene im Quarz."
1864. Bernard.—*C. R.* lviii. p. 1153; lix. p. 352. "Mémoire sur la détermination des longueurs d'onde des raies du spectre solaire au moyen des bandes d'interférence."
1864. Ditscheiner.—*Ber. d. Wien. Acad.* 50. Heft ii. p. 296. "Bestimmung der Wellenlänge der Fraunhofer'schen Linien des Sonnenspectrums."
1864. Ångström.—*Pogg. Ann.* cxxiii. p. 489; *Æfevers, af Förhandl.* (1863) p. 41. "Neue Bestimmung der Länge der Lichtwellen nebst eine Methode auf Optischen Wege die fortschreitende Bewegung des Sonnensystems zu bestimmen."
1866. Ditscheiner.—*Ber. der Wien. Acad.* 52. Heft ii. p. 289. "Eine absolute Bestimmung der Wellenlänge der Fraunhofer'schen D Linien."
1868. Mascart.—*Ann. de l'Ecole Normale*, iv. p. 7. "Recherches sur la détermination des longueurs d'onde."
1868. Mascart.—*Ann. de Chim. et de Phys.* [4] xiii. p. 186. "Note sur différents travaux relatifs aux longueurs d'onde."
1868. Van der Willigen.—*Arch. du Musée Teyler*, i. pp. 1, 57, 280. "Mémoire sur la détermination des longueurs d'onde du Spectre solaire."
1868. Ångström.—Upsala, 1868. *Recherches sur le Spectre solaire*.
1871. Ditscheiner.—*Ber. d. Wien. Acad.* 63. Heft ii. p. 265. "Zur Bestimmung der Wellenlänge der Fraunhofer'schen Linien."
1879. Peirce.—*Am. Journ. Sci.* [3] xviii. p. 51. "Note on the Progress of Experiments for comparing a Wave-length with a Metre."
1884. Thalén.—Upsala, 1885. *Sur le Spectre du Fer obtenu à l'aide de l'arc électrique*, p. 18.
1886. Müller and Kempf.—*Publicationen des Astrophysikalischen Observatoriums zu Potsdam*, v. "Bestimmung der Wellenlänge von 300 Linien im Sonnenspectrum."
1886. De Lépinay.—*Journ. de l'h.* [2] v. p. 411. "Détermination de la valeur absolue de la longueur d'onde de la raie D<sub>2</sub>."
1887. Bell.—*Am. Jour. Sci.* [3] xxxiii. p. 167; *Phil. Mag.* [5] xxiii. p. 265. "On the Absolute Wave-length of Light."
1887. Kurlbaum.—Berlin, 1887. *Bestimmung der Wellenlänge einiger Fraunhofer'schen Linien*.

In general the determination of absolute wave-length involves two quite distinct problems: first, the precise determination of some quantity which is an exact function of the wave-length and some other linear dimension; and, second, the reduction of this dimension to terms of some recognized standard of length. The first process can be made to give relative wave-lengths with a very high degree of accuracy, and is in nearly every case more exact than the second, which constitutes the main difficulty of the investigation. It is because the diffraction-grating lends itself readily to linear

measurement that its use is preferable to the other interference methods, which involve usually the exact determination of a single very small linear quantity. The ingenious attempt of M. de Lépinaï\* to avoid this difficulty is interesting theoretically, but practically it involves a quantity even more uncertain than the average standard of length (the relation between the kilogramme and the metre), to say nothing of the experimental difficulties of the method. The angular measurements of nearly all the later investigators have been quite good enough to furnish very exact values of wave-length; but in every case it has been the measurement of the grating-space that has produced the manifold errors and discrepancies in the results. It has been the aim of the present research to investigate this fruitful source of errors, and, as far as possible, to avoid the difficulties springing from it.

In a previous paper† I briefly discussed the advantages of transmission- and reflexion-gratings. It only remains to add that further experience has convinced me that, not only are speculum-metal gratings far superior in brilliancy and sharpness of definition, but that it is possible, contrary to what one might suppose from their large coefficient of expansion, to rule them with almost perfect uniformity over a length as great as a decimetre. This large size, too, gives a great advantage in determining the grating-space; apart from the fact that speculum-metal has a coefficient of expansion not widely different from that of any one of the materials usually employed for standards of length, and that its temperature can be obtained with comparative ease.

### *Methods and Instruments.*

The plane-grating can be used for wave-length measurement in a variety of ways, according to the preference of the investigator or the arrangement of the spectrometer. Five tolerably distinct methods may be enumerated. The general relation between the wave-length and the angles of incidence and diffraction is

$$\lambda = s (\sin i + \sin (\phi - i)) \frac{1}{n};$$

where  $\lambda$  is the wave-length,  $s$  the grating-space,  $i$  and  $\phi$  the angles of incidence and diffraction respectively, and  $n$  the order of the spectrum observed. Making  $i=0^\circ$ , this at once

\* *Journ. Phys.* [2] v. p. 411.

† *Am. Journ. Sci.* [3] xxxiii. p. 167; *Phil. Mag.* [5] xxiii. p. 266.



becomes the ordinary formula

$$\lambda = \frac{1}{n} s \sin \phi,$$

which applies to the two methods of normal incidence; one in which the grating is kept accurately perpendicular to the collimator, the other in which it is kept perpendicular to the observing-telescope.

Next is the method used by Ångström, in which  $i$  is not reduced exactly to  $0^\circ$ , but measured and retained in the formula, the grating in this case being kept nearly perpendicular to the collimator. In this method a reading on the slit is necessary; and if  $\alpha$  and  $\alpha'$  are the readings on the circle, and  $M$  that on the slit, the working formulæ are:

$$\frac{\alpha + \alpha'}{2} - M = \delta \quad \text{and} \quad \frac{\alpha - \alpha'}{2} = \phi;$$

then, if  $i$  is, as before, the angle of incidence,

$$\lambda = \frac{1}{n} s \sin \phi \cos (i + \delta),$$

$$\sin i = \sin (i + \delta) \cos \phi,$$

$$\tan i = \frac{\cos \phi}{1 - \cos \phi} \delta.$$

In the fourth method also  $i$  is retained, but given a definite value. Putting the general formula in the form

$$\lambda = \frac{1}{n} 2s \sin \frac{\phi}{2} \cos \left( i - \frac{\phi}{2} \right),$$

the deviation represented by the angular term will evidently be a minimum when  $i = \frac{\phi}{2}$ . If, then, one observes in the position of minimum deviation,

$$\lambda = \frac{1}{n} 2s \sin \frac{\phi}{2}.$$

In the fifth method, collimator and observing-telescope are kept at a fixed angle with each other and the grating is turned. In this case, if  $\phi$  is the angle of deviation, and  $\theta$  the angle between the telescopes,

$$\lambda = \frac{1}{n} 2s \sin \phi \cos \frac{\theta}{2}.$$

These methods are general, and the choice between them is simply a question of the convenient application of the apparatus at hand. Probably the first and second methods are

the most generally useful, while the third is the most objectionable. The method of minimum deviation slightly increases the experimental difficulties, but often improves the definition of the gratings, and is capable of giving very exact results. The last method is applicable only when the spectrometer is so rigid as to ensure the permanence of the angle between the telescopes. When this condition is fulfilled, however, the method is very valuable, since it reduces the moving mass to a minimum and allows the method of repetition to be readily used.

In the present research, for the work with glass gratings, the second method was selected as best suited to the arrangement of the spectrometer. This was a very good instrument by Meyerstein. The circle is 32 centim. in diameter, divided on silver to 6', and reading by two microscopes directly to 2'' and by estimation easily to within 1''. The collimating- and observing-telescopes are of 4 centim. clear aperture and about 35 centim. focal length, well corrected and firmly supported.

For the second part of the work, with speculum-metal gratings, it was desirable to use gratings of the largest size practicable, far larger than could be used on the above-described instrument, both by reason of the small aperture of the telescope and the inability of the grating-holder to carry the requisite mass steadily. This part of the work was therefore carried out on a very large instrument, designed by Prof. Rowland especially for using gratings of the largest sizes as yet ruled. This instrument has virtually fixed telescopes, solidly clamped, with a small lateral range of adjustment, to a T-shaped casting bedded in cement, which in turn forms the top of a large brick pier resting on a stone slab.

The telescopes are of 16.4 centim. clear aperture and about 2.5 metres focal length, and the objectives are of excellent quality. Each telescope is fastened to an arm of the T, which has a total length of over 2 metres, and bears at the extremity of the shaft the spectrometer proper. This is an instrument by Schmidt and Haensch, having a circle 32 centim. in diameter divided to 6', and, as in the other spectrometer, reading by two microscopes directly to 2'', and by estimation to less than half that amount. The original central platform had been removed and replaced by a grating-holder large enough to carry, if necessary, a 6-inch grating. Such an apparatus limits one, of course, to the fifth method; but so rigid is the whole affair, that experience soon showed that the angle between the telescopes did not change by any appreciable amount. The circle, however, was not finely enough graduated, nor were the microscopes of sufficient power to

derive the fullest benefit from the size of the telescopes ; over and over again has the line in the spectrum appeared slightly displaced from the crosshairs, when no difference whatever could be detected in the micrometer-readings. However, there was gained the great advantage of using gratings of a decimetre in length, giving spectra of great brilliancy and superb definition, and which could be measured with vastly greater exactness than is possible with the small gratings generally employed.

### *Gratings.*

Four gratings have been used in my experiments—two of glass and two of speculum metal. The former are probably the best of the very few glass gratings that have been ruled on Prof. Rowland's engine. They are ruled on plane sextant mirrors of rather hard glass.

Grating I. contains 12,100 spaces in a length of very nearly thirty millimetres, the lines being nineteen millimetres long. It was ruled in Jan. 1884, at a temperature of  $6^{\circ}7$  C. ; gives spectra of excellent definition, quite free from ghosts or false lines, and having almost exactly the same focus on both sides of the normal.

Grating II. has 8600 spaces, with almost exactly the same length and breadth as I., is free from ghosts and false lines, and, like I., is very smoothly ruled, though it is somewhat inferior to I. in the matter of regularity. The definition is excellent, and the spectra alike in focus on both sides of the normal. It was ruled in Nov. 1884 at  $11^{\circ}6$  C.

Gratings III. and IV. are on speculum metal. The plates are five inches square and five eighths of an inch thick, and were worked plane with especial care. The ruled surface is of the same size in each, four inches long by two inches length of lines.

Grating III. was ruled in April 1885, at a very nearly constant temperature of  $10^{\circ}$  C. It contains 29,000 spaces, having very nearly the same grating-space as II. It is a phenomenal grating both in its superb definition and extraordinary regularity of ruling, and was selected from a large number because of its very unusual perfection. The focus of the spectra on each side of the normal is the same, and the ruling is flawless.

Grating IV. was ruled on the new dividing-engine just completed by Prof. Rowland, and was one of the first large ones completed. Although the new engine has even now not received the finishing touches, it has turned out a few gratings of remarkable excellence. One of these is IV., which was

ruled in Dec. 1887, at a constant temperature of  $17^{\circ}2$  C. It contains 40,000 spaces within the same dimensions as III., is equal to it in definition, and but very little inferior in regularity of ruling. It has very nearly the same focus on both sides of the normal, and the ruling is wonderfully even and perfect.

It should be noted that these four gratings are widely diverse, being ruled at different temperatures and under different conditions. I. and II. were ruled to widely diverse grating-spaces on different parts of the screw; III. was on speculum metal and with more than six times the ruled surface of I. or II.; and finally IV. was ruled to a new grating-space on a new dividing-engine. These differences may not favour close agreement in the experimental results, but they certainly serve to eliminate anything like systematic errors due to the gratings.

The above gives a general view of the gratings employed; but some further details will be mentioned in the second part of this paper in connexion with the determination of the grating-spaces.

### *On the Standards of Length.*

Very many of the discrepancies in the determinations of absolute wave-lengths are the direct result of uncertainty in the standard of length employed. The cases of Ångström and Van der Willigen have been already alluded to; and the same source of error is common to all other determinations. It seems, therefore, desirable to give at some length the various comparisons on which the wave-length as given by my experiments is based. Reserving for the present the actual measurement of the gratings, which is a comparatively simple matter, I will therefore discuss the standards directly employed, their relations to the *Mètre des Archives* as found by various comparisons, and finally the changes which have taken place in those relations since they were first determined.

The standards with which the gratings have been directly compared are two double decimetres on speculum metal, designated respectively  $S_1^a$  and  $S_2^a$ . They were graduated and compared by Prof. W. A. Rogers in 1885. The bar  $S_1$  is 23 cm. long, and bears near its edge the double decimeter  $S_1^a$ , subdivided to centimetres. The defining lines are less than  $1\ \mu$  in width and beautifully sharp and distinct.  $S_2$  is 27 cm. long, and is graduated in the same way, with lines of the same width. Both standards are of the same speculum metal, and are of very nearly the same mass, while the surfaces and graduation leave little to be desired. The



coefficient of expansion of these bars was very thoroughly investigated by Prof. Rogers, and was found to be

17.946  $\mu$  per metre per degree C.

The absolute lengths of  $S_a^1$  and  $S_a^2$  depend on long series of comparisons with Prof. Rogers's bronze yard and metre  $R_2$  and steel copies thereof. Upon the relation existing between  $R_2$  and the Mètre des Archives depends, then, the absolute value assigned to the wave-length of light, since the close agreement of the various series of comparisons executed by Prof. Rogers between  $R_2$  and the speculum-metal standards show that no sensible uncertainty exists in the relations between them.

The yard and metre  $R_2$  is of the alloy known as Bailey's metal, this being the material of the Imperial Yard and many other standards. The graduations are upon platinum-iridium plugs, the polished faces of which are in the plane of one surface of the bar when supported at its neutral points. The relation of the metre  $R_2$  to the Mètres des Archives rests on a very large number of comparisons made with two entirely independent secondary standards; the copper metre designated T, and the brass yard and metre designated C.S. A full account of these comparisons is contained in vol. xviii. of the Proceedings of the American Academy of Arts and Sciences.

The metre T is on platinum plugs in a pure copper bar and was traced and compared by M. Tresca in 1880, from the Conservatoire line-metre No. 19, the relation of which to the Mètre des Archives was very exactly known.

The yard and metre C.S. has its graduations on silver plugs in a brass bar. The yard was compared directly with the Imperial Yard in 1880, and the standard was then sent to Breteuil, where it was compared with the International Metre by Dr. Pernet.

There were thus two completely independent sources from which the relation of  $R_2$  to the Mètre des Archives could be obtained. The results derived by very elaborate comparisons with each of these were as follows:—

$$\left. \begin{array}{l} \text{From T} \quad R_2 - A_0 = +1.5\mu \\ \text{From C.S.} \quad R_2 - A_0 = +1.1\mu \end{array} \right\} \text{at } 16^{\circ}.67 \text{ C.,}$$

where  $A_0$  is the Mètre des Archives. In addition to the very close agreement of the above, further evidence was obtained by deriving the relation between the yard and metre from  $R_2$ , the yard  $R_2$  having been exactly determined by

comparisons with C.S. and with "Bronze 11," one of the primary copies of the Imperial Yard, which had been recompared with that standard in 1878.

From the comparisons of  $S_1$  and  $S_2$  made in 1885 the following values of those standards were deduced:—

$$S^a_1 + 0.98\mu = \frac{1}{5}A_0, \text{ and}$$

$$S^a_2 + 0.2\mu = \frac{1}{5}A_0.$$

Hence  $S^a_2 = S^a_1 + 0.78\mu;$

and for the first decimetre were found the relations:—

$$Dm_1 S^a_1 + 0.05\mu = \frac{1}{10}A_0,$$

$$Dm_1 S^a_2 - 0.01\mu = \frac{1}{10}A_0,$$

Whence  $Dm_1 S^a_2 = Dm_1 S^a_1 + 0.06\mu.$

On these equations were based the results embodied in my former paper. In the latter part of May 1887, these standards were very carefully compared with each other, and with a speculum-metal bar graduated by Prof. Rowland, as I desired to take one or more of the standards to Berlin during the summer in order to get a comparison with the standard used by Müller and Kempf.

The results of this examination were of a somewhat startling character, as follows:—

$$S^a_2 = S^a_1 + 1.2\mu, \text{ direct,}$$

$S^a_2 = S^a_1 + 1.1\mu$ , through the Rowland bar designated  $R_B$ . Also,

$$Dm_1 S^a_2 = Dm_1 S^a_1 + 1.7\mu, \text{ through } R_B.$$

In 1885 Rogers had found for the relation between the two decimetres of each bar:—

$$Dm_2 S^a_1 = Dm_1 S^a_1 - 0.56\mu,$$

$$Dm_1 S^a_2 = Dm_2 S^a_2 + 0.46\mu.$$

I now found for the same quantities:—

$$Dm_2 S^a_1 = Dm_1 S^a_1 + 0.64\mu, \text{ direct,}$$

$$Dm_2 S^a_1 = Dm_1 S^a_1 + 0.60\mu, \text{ from } R_B,$$

$$Dm_1 S^a_2 = Dm_2 S^a_2 + 1.60\mu, \text{ direct,}$$

$$Dm_1 S^a_2 = Dm_2 S^a_2 + 1.65\mu, \text{ from } R_B.$$

All these relations being for  $16^\circ.67$  C.

The standard  $S^a_2$  was taken to Berlin during the summer; and through the kindness of Dr. Nieberding, Director of the Normal Aichungs Commission, I was enabled to have it compared with  $R_{78}$ , the standard metre to which the wave-length

measurements of Müller and Kempf, and Kaulbaum had been referred. From this comparison was derived the relation:—

$$S^a_2 - 1.68\mu (\pm 0.15\mu) = \frac{1}{5}A_0.$$

On returning to Baltimore, the first step was to re-determine the length of  $S^a_2$ . A series of comparisons was therefore instituted between it and the steel yard and metre  $A_4$ , the relation of which to  $R_2$  was accurately known,  $A_4$  having been traced and determined by Prof. Rogers, and furnished by him to the Johns Hopkins University. Only half of this standard is subdivided to decimetres; but a series of comparisons with the various pairs of decimetres gave the relation

$$S^a_2 + 1.3\mu = \frac{1}{5}A_0.$$

This result taken together with the relations found between  $S^a_1$  and  $S^a_2$  made it tolerably clear that a change had taken place in the speculum-metal standards; and to obtain a further confirmation, Prof. Rogers kindly consented to give them a rigid examination and again compare them with all attainable accuracy to  $R_2$ . His results for  $S^a_2$  were as follows:—

$$\begin{aligned} Dm_1 S^a_2 &= Dm_2 S^a_2 + 1.70\mu, \\ S^a_2 + 1.0\mu &= \frac{1}{5}A_0. \end{aligned}$$

There is no escape from the conclusion, therefore, that the speculum-metal bars  $S_1$  and  $S_2$  have changed both in absolute length and the relative lengths of their parts. Here are two bars of the same shape, mass, material, and constant of expansion. Each had the relation between its halves determined in the early part of 1885. Two years later these relations are found to have changed by at least  $1\mu$ ; and an independent determination by the original observer confirms this result in the most unequivocal way. Further, the original observer recom pares one of these standards with the standard from which it was originally determined, and finds a change of  $1\mu$ .

It should be borne in mind that with the comparator used by me in this work,  $1\mu$  is completely outside of any possible errors of observation. The microscope used was especially made for micrometric work, and has a power of two hundred and fifty diameters, while one division of the micrometer equals  $0.28\mu$ . The average error of a single comparison between two decimetres is rarely greater than  $0.1\mu$ , while the temperature of the observing vault can be kept for several days constant within  $0^\circ.5$  C., and during a day's observations

usually remained constant within half that amount. The bars under comparison were side by side, symmetrically placed with reference to the illumination, and were at temperatures very near to  $16^{\circ}67$ , at which they were standard.

The facts, then, concerning the speculum-metal bars are these:—In about two and a half years  $S_2^a$  has shortened by very nearly  $1.0\mu$  and  $S_1^a$  by a little over that amount. In  $S_2^a$  this change has taken place exclusively in the last decimetre, and in  $S_1^a$  it has been confined to the first decimetre.

The apparent slight increase in  $Dm_1S_2^a$  and  $Dm_2S_1^a$  I do not regard as beyond the effect of the experimental errors. The changes in the lengths of the subdivisions of these standards are very curious, and some explanation may be offered by the fact that the bars were cast in a nearly vertical position and annealed in sawdust, a method hardly sufficient for a material so strongly crystalline as speculum metal. I think, however, one is justified in drawing the conclusion that speculum metal, so tempting on account of its beautiful surface and the exquisite sharpness of the graduations drawn upon it, is a material thoroughly unsuitable for standards of length by reason of its tendency to change with time. I have thus entered into somewhat minute details in the case of these bars, because the whole question of changes in standards of length is in a somewhat unsettled state, and it seems desirable to put on record this case, which has been investigated with more than ordinary care by both Prof. Rogers and myself, and in which the changes found have taken place within a comparatively short time.

It is quite well known that in 1855 this question was raised by Mr. Sheepshanks, then engaged in constructing the new British standards. Discrepancies amounting sometimes to 2 or  $3\mu$  appeared in his measurements; but after a considerable amount of study, these differences appeared to be too irregular to be fairly ascribable to actual changes. Slight variations of temperature, especially when the standards compared were of different materials, the lagging of the real temperatures of the bars behind the thermometer indications, and particularly the effect of coarse and sometimes unsymmetrical defining lines, are perhaps enough to account for the observation.

The work, however, done on the U.S. bar "Bronze 11," as reported in the report of the Coast Survey for 1877, seems to show genuine change in that standard.

A long series of comparisons with the Imperial Yard and its copies in 1878, showed systematically a shortening relative to the Imperial Yard of over  $4\mu$ . Although further measure-



ments have tended to somewhat lessen this discrepancy, it seems to be sufficient, considering the fact that "No. 11" and the Imperial Yard are of the same shape, material, and mass, and were compared on the same apparatus as during the original comparisons in 1857, and at nearly the standard temperature, to establish the fact of a real change. While 3 or  $4\mu$  is absolutely a small quantity, its systematic appearance under conditions almost identical with those of the original measurement can hardly be ascribed to experimental errors. The other cases cited in the above-mentioned paper tend to confirm this conclusion.

The gradual and sometimes very irregular changes that are known to take place in both the bulbs and stems of thermometers would lead one to expect that glass standards of length would be liable to similar changes, though probably far less in amount. It was, therefore, with special interest that I examined glass Decimetres III. and IV. belonging to the Coast Survey, and used by Peirce in his wave-length measurements. These scales are on plate glass, of the same dimensions, and having coefficients of expansion not widely different. A series of comparisons made at a nearly constant temperature of  $16^{\circ}.5$  C. gave the direct relation

$$\text{III.} = \text{IV.} + 2.1\mu.$$

While the same relation deduced from Peirce's direct comparison, by applying the coefficients of expansion assigned by him, is

$$\text{III.} = \text{IV.} + 1.3\mu.$$

The defining-lines on both standards are fine and sharp; and unless Peirce's coefficients are grossly in error, the evidence of change between 1879 and 1887 is very strong indeed.

Having now the exact present relation of  $S^a_2$  to the original standard  $R_2$ , it remained only to investigate the difference between this result and the length of  $S^a_2$  as deduced from the Berlin comparisons. I have been unable to obtain the details concerning  $R_{78}$ , the standard used in these comparisons, but it was determined by comparison with the standard metre of the International Bureau. The comparisons of  $S^a_2$  with  $R_{78}$  were carefully made by two observers, and it is probable that the result represents the relation between these standards with considerable exactness. It should, however, be borne in mind that the microscopes had each a power of only 50 diameters, and that the bars in question are of very different material and mass, thus giving a chance for small errors due to varying temperature.

It is possible, however, to check this result by referring  $S_2^a$  to the Berlin platinum standard through the medium of the Coast Survey metre "No. 49." This latter standard was compared in 1876 with metre 1605 and directly with the platinum metre. The details are given in Prof. Foerster's report contained in the Report of the Coast Survey for 1876. The result of the direct comparison was

$$Pl - "49" = +24.4\mu.$$

But now Prof. Rogers has compared  $R_2$  with "49," obtaining in terms of the assumed length of  $R_2$

$$"49" = A - 19.3\mu;$$

the assumed value of  $R_2$  was

$$R_2 = A + 1.3\mu.$$

Hence we have

$$R_2 - "49" = 20.6\mu,$$

from which follows

$$Pl - R_2 = 3.8\mu.$$

If now the equation between  $Pl$  and the *Mètre des Archives* established by direct comparison in 1860 be correct,

$$A_0 - Pl = -3.01\mu.$$

And therefore

$$R_2 - A_0 = -0.8\mu,$$

a result which is in close accordance with those derived from the *Conservatoire* metre and Type I. of the International Bureau by means of the Standards T. and C.S.

In my final determination of wave-length, I have used the mean value of  $S_2^a$  as derived by the foregoing methods. Collecting equations,

$$S_2^a + 0^{\mu}.96 = \frac{1}{5} A_0. \quad \text{From T.}$$

$$S_2^a + 1^{\mu}.04 = \frac{1}{5} A_0. \quad \text{,, C.S.}$$

$$S_2^a + 1^{\mu}.40 = \frac{1}{5} A_0. \quad \text{,, "49."}$$

$$S_2^a - 1^{\mu}.68 = \frac{1}{5} A_0. \quad \text{,, } R_{78}.$$

Giving to the equations derived from C.S. and  $R_{78}$  twice the weight of the others, we have finally,

$$S_2^a + 0^{\mu}.27 = \frac{1}{5} A_0.$$

I have given the relations derived from C.S. and  $R_{78}$  double weight because these standards have been compared directly with the standard of the International Bureau, which now, probably, should be regarded as the ultimate standard of

reference. Especially is this true, since it is rumoured, apparently not without foundation, that the *Mètre des Archives* is at present, for some unassigned reason, undesirable as a direct standard of reference.

It is unfortunate that there is not more general uniformity in the material, shape, and mass of standards of length. Difference in these particulars are fruitful sources of error in comparisons, and when one adds to this the trouble arising from bad defining-lines and imperfect focus, the wonder is that the results are as good as they usually are. It is hard to say what material is least liable to changes, but it is quite certain that substances of crystalline structure, and alloys of which the physical properties are largely dependent on a nearly definite composition, should be avoided. Probably pure platinum, silver, and copper, annealed with the utmost care, and kept for some years before final graduation, are less likely to change than any other material which we know. For short standards, possibly bars of native copper, prepared with as few strains as possible, would give the closest possible approximation to a material which has arrived at a permanent state.

Physical Laboratory,  
Johns Hopkins University,  
Feb. 22, 1888.

---

XXXI. *Observations on the Height, Length, and Velocity of Ocean Waves.* By HON. RALPH ABERCROMBY, *F. R. Met. Soc.\**

THE interest in ocean waves has so much declined in recent years, that physicists have perhaps scarcely realized how much more easily measurements can be taken now than formerly.

In the old days wave-heights could only be ascertained, more or less, by estimation; while the length and speed could only be determined by a common watch. Now-a-days the aneroid can easily measure small vertical heights to within one or two feet; while the fly-back chronograph enables time to be measured to the  $\frac{1}{10}$ th second, without taking the eye for one moment off the object to be watched.

The following observations were taken on board the S.S. 'Tongariro,' in various parts of the S. Pacific between New Zealand and Cape Horn, in the month of June 1885.

Height was measured by a  $4\frac{1}{2}$ -inch aneroid with a very

\* Communicated by the Physical Society: read February 25, 1888.

open scale, divided to the  $\frac{1}{160}$ th inch ; so that the readings could be taken at a glance to 0.025 inch, or, when time allowed, to 0.020. The instrument is an extremely good and accurate barometer. The altitudes were all calculated on the simple assumption that a difference of 1 foot in height is given by a difference of 0.001 inch of pressure. Any error which could arise between this reduction and that by a more rigorous method would be far less than the other errors of observation.

So far the observation was simple enough ; but the great difficulty arose when the height of the eye above the sea-level, at the moment of observation, had to be estimated. For instance, when the barometer was at its lowest point the surface of the water might be 10 feet below the eye ; but when the crest of the wave rushed past, the height might be reduced to 1 or 2 feet. Here I had to trust to my own estimation by eye, aided by a few rough measurements with a piece of string down the ship's side. The principal uncertainty of the results depends on errors of this estimation. I do not think that the aneroid errors would ever be more than 2 or 2.5 feet, while those of estimation might be at least 2 feet either way.

Length and velocity were determined by standing in a suitable position with a chronograph, and measuring (1) the interval between the time when two successive crests reached the stern, and (2) the time that the crest of the first wave took to run the length of the ship. Then the length of the ship, her speed, and course relative to the direction of the wave's progress being known, the velocity and length of wave could be readily calculated by the following formulæ :—

Let  $t$  = time from crest to crest, in seconds ;

$T$  = time of first crest running the length of the ship,  
in seconds ;

$l$  = length of wave, in miles ;

$v$  = velocity of wave, in miles per hour ;

$k$  = speed of ship, in miles per hour ;

$L$  = length of ship, in fractions of a mile ;

$\theta$  = angle between course of ship and direction of wave-progress.

Then, for a following sea,

$$v = L \cos \theta \frac{3600}{T} + k \cos \theta ;$$

$$l = (v - k \cos \theta) \frac{t}{3600}, \text{ in miles,}$$

$$= (v - k \cos \theta) \frac{t}{3600} \times 5280, \text{ in feet.}$$



The calculations were made with sufficient accuracy by means of a slide-rule.

It is manifest that it is only possible to obtain an approximation to any of the desired data. Standing near the stern it is not easy to observe the precise moment when the crest reaches the bow; two successive waves rarely run in exactly the same direction; and, with a heavy following sea, the ship yaws about so much that the angle between her course and that of the waves can only be estimated approximatively.

I found great advantage in using cards for the original records, as observations must often be made in rain, sleet, spindrift, and frequently at temperatures near the freezing-point, when the fingers are apt to get benumbed.

The following are some of my most interesting results :—

June 8, 1885. lat.  $47^{\circ}$  S., long.  $175^{\circ}$  W.—Sea too irregular to measure individual wave-heights or lengths, but the barometer indicated about 12·5 feet for the vertical motion of the point of observation below decks. Then, pretty constantly, the surface of the sea was about 7 feet below the part-hole in the troughs, but only 1 foot at the crest. This would give the waves an average height of about 18·5 feet.

The velocity of the waves could be got much better. Five observations gave the accordant speeds of 20, 28, 31, 33, and 30 miles an hour, or an average of 30·2 miles.

The following data were used in the calculation :—Ship, length 380 feet; course S.E.; Speed 14 knots. Sea running to S.S.E., and therefore following.

June 10, 1885. Lat.  $51^{\circ}$  S., long.  $160^{\circ}$  W. Height.—I found it impossible single-handed to estimate the height of the water at the moment the aneroid was read, so I took a constant difference of 6 feet for the difference of the height of the eye at trough and crest. The following is an example of the readings :—

Trough.	Crest.	Difference.	Diff. height eye, trough and crest.	Estimated height, in feet.
28·955	28·975	·020	6	26
·97	·985	·015	6	21
·9675	·985	·0175	6	23·5
·960	·980	·020	6	26

These waves were not consecutive, and the difference between the extreme heights is a good deal larger than the

height of any single undulation. The highest reading of the series (not given here) was 28·99, the lowest 28·955, which would give a difference in height of 35 feet.

The length and velocity were measured on deck just before the heights, under the following conditions :—

Ship: length 380 feet, course E.S.E., speed 14 knots. Sea running to S.E., and therefore following.

The following are some of the best observations :—

Time of crest, running length.	Time between two crests.	Velocity. Calculated.	Length. Calculated.
12·5	19	32 miles.	507
11·0	15	35 „	470
9·0	...	39·5 „	
16·0	17	28·5 „	358

This sea would have been logged as 6 or 7 on the ordinary scale of 0–8. The wind was blowing a moderate to hard gale from N.W. with heavy squalls, and was logged 7 on Beaufort's scale of 0–12. During some of the squalls, with thunder and lightning, the force rose to 8; and though the ship was never nearly pooped, we managed to split a topsail. I think this might be taken as a fair average sea in the S. Pacific. The waves were far too irregular to allow of any attempt being made to determine the ratio of height to length or velocity.

July 16, 1885. Lat. 55° S., long. 105° W.

Trough.	Crest.	Difference.	Diff. of height eye.	Estimated height.
29·38	29·4025	·0225	6	28·5 feet.
·38	·42	·040	6	46 „
·3775	·40	·0225	6	28·5 „
·38	·41	·030	6	36 „

The readings in the trough belonging to this series are remarkably uniform.

Length and velocity were measured from deck under the following conditions :—Ship, length 380 feet ; speed, 14 knots ; course to E. ; sea from W., and therefore right aft.

Time of crest, running length.	Time between two crests.	Velocity. Calculated.	Length. Calculated.
12.5	14.25	35.5	445
12.5	16.5	35.5	485
8.0	16.0	47.5	765

This was the heaviest sea we encountered during the whole voyage. The wind was blowing a hard gale from S.W., with squalls of hail and sleet. The sea was certainly high, but did not appear excessively so to the eye. The ship broached to and lay in the trough of the sea for nearly half an hour while a gland in the engine-room was being packed ; but no harm was done beyond splitting a topsail. The ship rolled tremendously, but no seas swept over her.

Under these circumstances the height of 46 feet, given by one set of observations, seems excessive. The actual vertical lift of the cabin was undoubtedly 40 feet ; for, as I noticed it at the time, there is not a mistake in the records. Any error in the true height must come from the estimate of 6 feet for the difference of the height of the eye on the crest and in the trough.

It will be noticed that the relation of length and velocity to height is very irregular ; but this is due to the character of the waves, and not to errors of observation. On all the days the waves were running irregularly. We never saw crests nearly a mile long chasing one another with a well-defined trough between them ; but the seas were so confused, that sometimes after one big crest nothing followed but some small waves. There was nothing to call a cross sea ; but there were many series of waves of different lengths running pretty much in the same direction, which were constantly interfering with one another. An eye estimate of height is always delusive, but I was surprised that the waves measured as much as they did. I have seen a heavier sea in the Atlantic ; so that if we take only 40 feet as the highest of this series, it is perfectly certain that much greater heights are sometimes attained.

Fitzroy says that, even during many years spent at sea, a

man only observes really high waves once or twice during a lifetime; and this view is confirmed by every sailor I have conversed with. Taking my own measurements as representing only ordinary seas, I am certain that 60 feet at least from trough to crest must be attained by exceptional waves.

It may be interesting to compare the results given here with those obtained by other observers. The following figures, except those in the last line, are taken from Dr. Krummel's *Oceanographie*.

Table of the Maxima Dimensions of Waves.

Authority.	Locality.	Height, in feet.	Velocity, m.p.h.	Length, in feet.	Period, in seconds.
Lieut. Paris .....	Indian Ocean.	.....	33.6	374	7.5
Admiral Molter...	.....	.....	80.0	2703	23.0
Captain Ross* ...	Near C. Good Hope.	22.97	90.0	1902	
Captain Chûden..	33° S., 107° W.	33-36	.....	984-1312	
D'Urville .....	.....	98.0			
Scoresby .....	.....	42.0			
'Novara' .....	.....	36.0			
'Challenger' .....	.....	23.0			
Abereromby .....	S. Pacific.	46.0	47.5	765	16.5

It is manifest, from an inspection of the above table, that the discrepancies are enormous. I cannot but think that the extreme lengths recorded by Admiral Molter and Captain Ross must have not allowed for the interference of following waves. No doubt such lengths might be observed between two notable crests, but most probably there would be some smaller undulation between. I never took the time between two crests unless they manifestly belonged to a simple wave.

The three sets of observations before noticed were taken on the only three days on which at all big waves were met with. They serve to show the difficulties of wave-measurement; but the greatest obstacle is the uncertainty of meeting first-class specimens. If ever a wave-measuring party should be organized, I think it should be arranged as follows.

Three observers, A, B, and C, would be necessary. A would command the party, say when the instruments were to be observed, note personally the height of the deck from the water, and enter all the readings on a suitable card. B would have a suitable aneroid, and confine his whole attention to that instrument. C would be furnished with two chronographs.

When a crest touched the stern of the ship, A would give

\* Dr. Krummel's figures do not agree with those given in other places for Capt. Ross's results.



a signal, on which B would read his aneroid, while C started both his chronographs. A would first note the height of the deck by marks on the ship's side; and, if need be, read a simple clinometer to allow for the roll of the ship before entering the records of his assistants. When the crest reached the bow, A would give another signal for C to stop one chronograph. In the trough, B would read his aneroid, while A noted the height of the water; and, finally, as the next crest came on, B would read his aneroid, C would stop his second chronograph, while A noted the height of the water and entered all the records.

By this means, and with a careful selection of tolerably undeformed waves, I think that the measurements of undulations could be much more satisfactorily obtained than heretofore; and I only regret that the means at my disposal did not enable me to do more towards this important line of research.

#### SUMMARY.

The results of this paper may be summarized as follows:—

Several sets of observations between New Zealand and Cape Horn, with an aneroid barometer and chronograph, gave for the largest waves a height of 46 feet, a length of 765 feet, a speed of 47 miles an hour, and a time-period of 16·5 seconds.

As nothing but the ordinary heavy weather of these latitudes was experienced, it is certain that waves must sometimes attain a height of at least 60 feet.

Really big seas are of very rare occurrence.

The great discrepancies in the observed elements of waves given by different observers is doubtless due to the varying lengths of every series of undulations, which therefore always make a more or less confused sea.

---

#### XXXII. *The Integral Weight of Water.*

*By T. STERRY HUNT, LL.D., F.R.S.\**

IN a paper on Chemical Integration, published in the American Journal of Science for August 1887, and reprinted in the 'Chemical News' of September 23 and 30, it was said that, in comparing the densities of liquid and solid bodies with those of known gaseous species, such as water-vapour and carbon dioxide, "or in the last analysis, with the density of the hydrogen unit . . . we get the specific gravity of these bodies, the dyad integer of hydrogen at 0° and 760 millim. ( $H_2=2\cdot0$ ) being unity." Subsequently, in a paper

\* Communicated by the Author.

on Integral Weights in Chemistry, in this Magazine for October 1837, it was stated that a litre of hydrogen gas "at  $0^{\circ}$  and 760 millim. being assumed as a unit of volume for all species, the weight of a litre of any other gas or vapour at the standard temperature and pressure is its integral weight. In like manner the integral weight of a liquid species is the weight of the same volume at its boiling-point under a pressure of 760 millim. . . . . The weights thus obtained for equal volumes of the various liquid and solid species, as well as for the gaseous species, are evidently the specific gravities of these species; that of hydrogen at the standard temperature and pressure being unity ( $H_2=2.0$ ). They are at the same time the integral weights of the species compared."

Notwithstanding this clear statement in both papers that it is hydrogen gas at  $0^{\circ}$  and 760 millim. which is to serve as the unit of specific gravity alike for gaseous, liquid, and solid species, the reader will find in these papers, and also in the first edition of the author's 'New Basis for Chemistry' (1887), an error in the subsequent calculations. The problem having been approached from the comparison of the weights of equal volumes of liquid water at  $0^{\circ}$  and  $100^{\circ}$ , and of water-vapour at  $100^{\circ}$  and 760 millim., by an inadvertence (until now unperceived) the weights alike of hydrogen gas and of water-vapour at the latter temperature were substituted for their weights at  $0^{\circ}$  and 760 millim., thus leading to a grave error in the figure given for the integral weight of liquid water, and of bodies for which it serves as the unit of specific gravity, and making it equal to 29244. In fact, however, taking as the unit of weight that of the litre of hydrogen gas at standard temperature and pressure ( $0^{\circ}$  and 760 millim.), and comparing it with that of liquid water at  $100^{\circ}$  (its temperature of formation at 760 millim.), when a litre of it weighs 958.78 grammes, we have

$$0.0896 : 958.78 :: 2 : x = 21400.3.$$

This value is, then, alike the specific gravity of the liquid on the hydrogen basis, and its integral weight, which, if we take  $H_2O=17.96$ , corresponds very closely to 1192 ( $H_2O$ ) = 21408; ice being probably 1094 ( $H_2O$ ), calcite 584 ( $CCaO_3$ ), and aragonite 630 ( $CCaO_3$ ). While the writer regrets this error in calculation, made in direct contradiction to the principles laid down by him in both of the papers cited, it will be seen that its correction in no way affects their argument, which he hopes to develop further at an early day.

Washington, D.C.,  
February 22, 1888.

XXXIII. *Note on the Electromotive Force in Moving Conductors.*

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

I SHOULD like to be allowed to call the attention of those interested in the theory of electrodynamics to the method of estimating the E.M.F. in moving conductors generally employed by writers on this subject. The notation I use will require no explanation, as it is universally adopted.

In a closed circuit at rest it is known that the E.M.F. is proportional to the time-variation of the flux of magnetic induction through the circuit, *i. e.* across any surface bounded by the closed circuit, and therefore that it is equal to

$$-\int \left\{ \frac{dF}{dt} \frac{dx}{ds} + \frac{dG}{dt} \frac{dy}{ds} + \frac{dH}{dt} \frac{dz}{ds} \right\} ds,$$

taken round the circuit.

We cannot of course determine from this result with absolute certainty the E.M.F. in an unclosed circuit, or in each element of a closed circuit; but we know that our results will be consistent with the foregoing if we assume the  $x$  component in each conducting element at rest in a magnetic field to be

$$-\frac{dF}{dt} - \frac{d\psi}{dx},$$

and similarly for  $y$  and  $z$ .

If therefore the element were moving in a constant field at rest, the reasonable assumption would appear to be that the E.M.F. is

$$-\frac{dF}{dt} - \frac{d\psi}{dx};$$

where the  $\frac{dF}{dt}$  in this case is the time-variation of  $F$  at the

conducting element assumed at rest while the field, otherwise unaltered, moved, as a whole, with the reversed motion of the conductor, the *relative* motion of field and conducting element being the same, and  $\psi$  (whatever its meaning) being the same as before; because all our knowledge of motion can only be relative, and it is impossible to say whether the conductor or field is at absolute rest.

Instead of this, however, it is generally assumed that the  $x$  component of E.M.F. is in this case (moving conductor in constant field)

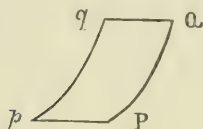
$$c \frac{dy}{dt} - b \frac{dz}{dt} - \frac{d\psi}{dx}.$$

Now  $c \frac{dy}{dt} - b \frac{dz}{dt}$  differs from  $-\frac{dF}{dt}$  (determined as suggested) by  $\frac{d}{dx} \left( F \frac{dx}{dt} + G \frac{dy}{dt} + H \frac{dz}{dt} \right)$ , so that the E.M.F. in this case, to be consistent with the preceding, should be

$$c \frac{dy}{dt} - b \frac{dz}{dt} - \frac{d\psi'}{dx} - \frac{d\psi}{dx},$$

where

$$\psi' = F \frac{dx}{dt} + G \frac{dy}{dt} + H \frac{dz}{dt},$$



The interpretation of this  $\psi'$  was given by Dr. Larmor in a paper on induced currents, in this Magazine, January 1884; viz. if in the above figure the conducting element PQ moves to pq in time  $\delta t$ , the total number of tubes of force cut by it in this time is measured by the flux across the area PQqp; and this, divided by  $\delta t$ , is generally taken to be the measure of E.M.F. across the moving element PQ; differing from the  $\frac{dF}{dt}$ , as above defined, by the differences of integrals of  $Fds$  along Qq and Pp, i. e. by

$$\frac{d}{dx} \left( F \frac{dx}{dt} + G \frac{dy}{dt} + H \frac{dz}{dt} \right).$$

There is no more reason for assuming the magnetic E.M.F. to be measured by the total number of tubes of force thus cut, than by the differences of line-integral of the vector-potential; but it would seem better to assume in both cases the same means of estimating the E.M.F., and to keep the same meaning of  $\psi$ .

Thus, if  $\psi$  be regarded as the potential of free electricity in the one case, it should be so also in the other. It would, I think, prevent some confusion in the mind of the student. For instance, with a substance at rest the equations are

$$P = -\frac{dF}{dt} - \frac{d\psi}{dx},$$

$$Q = -\frac{dG}{dt} - \frac{d\psi}{dy},$$

$$R = -\frac{dH}{dt} - \frac{d\psi}{dz};$$



whence  $4\pi\rho$ , or

$$\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz} = -\nabla^2\psi,$$

where  $\rho$  is the volume-density of free electricity.

But with a moving substance the equations usually employed are :

$$P = c \frac{dy}{dt} - b \frac{dz}{dt} - \frac{d\psi}{dx},$$

$$Q = a \frac{dz}{dt} - c \frac{dx}{dt} - \frac{d\psi}{dy},$$

$$R = b \frac{dx}{dt} - a \frac{dy}{dt} - \frac{d\psi}{dz};$$

whence  $4\pi\rho$ , or  $\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz}$ , becomes, as pointed out by Professor Niven,

$$-\frac{1}{4\pi} \left( u \frac{dx}{dt} + v \frac{dy}{dt} + w \frac{dz}{dt} \right) + 2(\alpha\omega_1 + \beta\omega_2 + \gamma\omega_3) - \nabla^2\psi,$$

$\omega_1, \omega_2, \omega_3$  being the component angular velocities of the substance. But the first two terms on the right-hand side are (as may readily be verified) together equal to

$$\nabla^2 \left( F \frac{dx}{dt} + G \frac{dy}{dt} + H \frac{dz}{dt} \right),$$

and would not have appeared if we had taken for our equations

$$P = c \frac{dy}{dt} - b \frac{dz}{dt} - \frac{d}{dx} \left( F \frac{dx}{dt} + G \frac{dy}{dt} + H \frac{dz}{dt} \right) - \frac{d\psi}{dx},$$

&c.

That is to say, if we had taken the E.M.F. in the element as if that were at rest and the field moving, a twofold simplification would have resulted :—

(1) The E.M.F. would have been made to depend upon *relative* motion only, as it seems *à priori* reasonable that it should do ; and

(2) The symbol  $\psi$  would have the same meaning in both cases, viz. the potential of free electricity in the field.

Yours faithfully,

Berkswell Rectory, Coventry,  
February 15, 1888.

H. W. WATSON.

XXXIV. *Sketch of a Stratigraphical Table, chiefly for Western Europe.* By JOHN YOUNG, M.D., Professor of Natural History and Honyman Gillespie Lecturer on Geology in Glasgow University\*.

THE accompanying table is an attempt to apply and extend the philosophical principle underlying a remark made many years ago by Sir A. C. Ramsay, that the Carboniferous Limestone was an episode in British Geology. I discussed the matter tentatively in 1876 (Brit. Assoc. Report, Glasgow, Address to Section C), and now submit a fuller scheme. The geology of Western Europe is the record of a border warfare between the Atlantic on the one hand and the Europeo-Asiatic continent on the other, the advance and retreat of ocean and land giving rise to a series of dissolving geographical views, not one of which can be definitely fixed. Of ocean and of continent we have little knowledge: of the debatable line between them our whole geological knowledge is made up. The index map of colours published by the Geological Survey is commonly made the basis of calculations of geological time, as if every superposed block of colour represented a distinct epoch. My table is an attempt to put in new shape the common stock of geological teachers, that land and sea are contemporaneous, and that a true narrative should not place all the strata of one region—say, Western Europe—on the top of each other, but should allow for the contemporaneous occurrence of sea and land, of thick and of thin deposits, on the inclined plane from the coast down to the floor of the adjacent sea. Sir A. C. Ramsay gave to Mr. Darwin a summary statement of British Geology, from which it seemed a fair arithmetical inference that the thickness of the sedimentary crust is 20 miles. I do not know for what purpose that statement was prepared; but I do know that it has been used by subsequent writers as if the “onion-coat theory of the earth’s crust” (to use Herbert Spencer’s graphic phrase) were still accepted. The right-hand side of my table is that of the permanent land-area, the left that of the permanent Atlantic Ocean. The left column shows the successive incursions of the ocean; the right column indicates the periods during which elevation carried the land, in whole or in part, to or towards the present western confines of Europe; in the interval are recorded the estuarine and fluvio-lacustrine conditions. I need not point out in detail how such a table tends to demonstrate that the duration

\* Communicated by the Author.

N  
A  
T  
I  
O  
N  
A  
L  
A  
N  
T  
A  
T  
I  
O  
N  
A  
L

E  
U  
R  
O  
P  
E  
O  
-  
A  
S  
I  
A  
T  
I  
C  
C  
O  
N  
T  
I  
N  
E  
N  
T

Clays.	}		Last Glaciation.	
Sands.		BOULDER - CLAY.	FOREST-	
Gravels.		CRAG DEPOSITS.	BED &c.	
		U P P E R	M I O C E N E S.	
		Vienna.	Touraine.	Switzerland. [Sivalik.]
[NUMMULITIC.]		ENGLISH EOCENE.		LOWER MIOCENE.
Europeo-Asiatic.				
CHALK.	NEOCOMIAN.		PURBECK—WEALD.	
		J U R A S S I C.		
		S. England.	M. & N. England & Hebrides.	N. Scotland.
[MUSCHELKALK &] KEUPER.		TRIASSIC.		
		Cheshire.		Scotland.
RHÆTIC.				
		PERMIANS.		
MAGNESIAN LIMESTONE.				
		COAL-MEASURES.		
CARBONIFEROUS LIMESTONE.		CALCIFEROUS		
Derby.		Scotland & N. Ireland.		
		SANDSTONE.		
DEVONIAN.				
		OLD RED SANDSTONE.		
SILURIAN.				
		?		
CAMBRIAN.				
		?		
PRECAMBRIAN.				
Deeper Sea.	Shallow.	Estuarine.	Fluvio-lacustrine.	Terrestrial.

of geological time is not so great as is ordinarily assumed. There is no reason to believe that the time required for the deposit of thick strata must be longer, proportionally to the number of feet, than that required for the deposit of thin. If the thick strata had been laid down in waters as deep as the average of the deeper areas of the Atlantic, vast time might perhaps have been required; but there is no evidence for such depths having ever been attained to the east of the present line of 100-fathom soundings off the British Islands. Even the Carboniferous and Cretaceous Limestones do not prove the existence of such deep waters; they only show that at these periods there was long-continued subsidence and continuous deposit in clear water. Prof. Huxley's homotaxis is thus applied within the compass of the European area; and, if the views embodied in this table are correct, the careful correlation of strata which has given so much occupation to geologists loses much of its importance. The palæontological differences are the variations of inhabitants of adjacent zoological areas on a sea-floor inclining westwards.

---

XXXV. *Experiments on Electrolysis.*—Part I. *Change of Density of the Electrolyte at the Electrodes.* By W. W. HALDANE GEE, B.Sc., Assistant Lecturer in Physics, and H. HOLDEN, B.Sc., Bishop Berkeley Fellow in Physics, of the Owens College, Manchester\*.

WHILST studying some electrolytic polarization phenomena with palladium electrodes in dilute pure sulphuric acid, a liquid was seen, *after a reversal of the current*, to flow downwards in streaks from the anode. Not being able to find any reference to the formation of streaks, for whose appearance the reversal of the current was necessary, it was decided to investigate their character. Further, it was thought that the occluded hydrogen might, on reversal of the current, unite with the nascent ion liberated at the anode, and thus effect chemical changes of an interesting character.

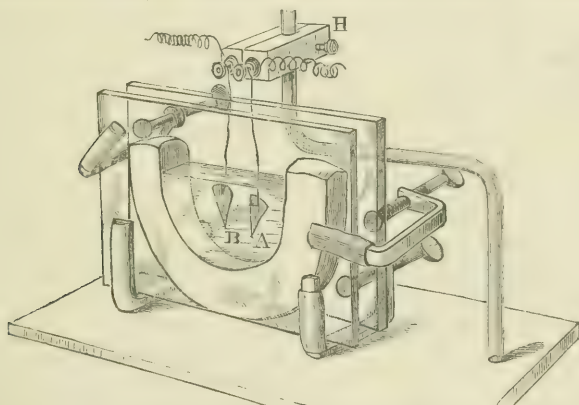
Some little care in observation and adjustment of the light

\* Communicated by the Physical Society: read February 25, 1888.

This is the first of a series of papers on Electrolysis and Electrolytic Polarization, descriptive of experiments made, during last year, at the Owens College Physical Laboratory. An abstract of the experiments made, to the end of August 1887, was submitted to the British Association Meeting at Manchester. We desire to acknowledge the assistance received up to that time from Mr. C. H. LEES, B.Sc., Derby Mathematical Scholar of the Owens College. His cooperation has since been discontinued, owing to absence at Strasburg.



is necessary in order to see the streaks, which, like the surrounding electrolyte, are colourless, and are only visible on account of the difference between their index of refraction and that of the main bulk of the electrolyte. A vessel with parallel sides of good plate-glass was employed, and a mirror used to reflect light obliquely into the cell. The arrangement which has been found to be most convenient is shown in the figure, and consists of a cell made of two pieces of plate-glass about 15 centim. square. A piece of india-rubber, 25 centim. long and 3 centim. square section, is bent in a semicircular form and clamped between the two glass plates by means of four iron screw-clamps. In this way a water-tight cell, about 1.5 centim. broad, is obtained which may readily be taken to



pieces for the purpose of cleaning. The electrodes have usually consisted of two pieces of palladium, about .05 centim. thick and with a surface of .5 square centim., fastened to platinum wires and supported by means of a convenient electrode-holder.

*Streaks obtained in dilute pure Sulphuric Acid.*

The electrodes were first heated to redness in order to drive out any occluded gas, and then cleaned by means of glass-paper. They were then placed, by means of the electrode-holder H, in the electrolytic cell containing dilute pure sulphuric acid. The current was sent from one electrode (A) to the other (B), which is thus the kathode, for a certain time and then reversed. On reversal no gas appears at first from B, which is now the anode, but streaks resembling a dense liquid are with careful observation seen flowing downwards from this electrode. After a time, depending on the size of the electrodes, the strength of the current, and its duration in

the first direction, evolution of gas at B begins, and the streaks simultaneously cease to be visible. The streaks are to be seen streaming from one electrode only at a time, and always descend from that palladium electrode which first serves as kathode and then as anode.

When platinum electrodes are substituted for the palladium ones, we have not been successful in obtaining the streaks; it is, however, only necessary that one of the electrodes should be palladium in order to obtain the streaks.

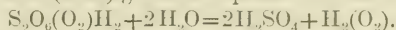
An experiment was made in order to ascertain the part which the reversal of the current plays in the formation of the streaks. The current was sent in one direction through the electrolyte, the palladium electrodes then taken out, washed, and their surface cleaned with glass-paper. On now reversing the current and replacing the electrodes, the streaks are quite as evident as if the electrodes had not been taken out. But if, on taking the electrodes out, they are heated to redness instead of merely having their surface cleaned, on replacing them and reversing the current the streaks are not seen, and gas appears immediately from the anode. We concluded from this experiment that the function of the current in the first direction was to fill with hydrogen that electrode at which the streaks appear in the second direction of the current; and that the reason why, after a time, the streaks ceased to be visible, and simultaneously gas began to be evolved at the anode, was that by this time all the occluded hydrogen had been used up. It was therefore thought probable that the streaks were formed by a combination of the occluded hydrogen with the ion (either  $\text{SO}_4$  or some of its components, such as oxygen) which is liberated at the anode in the second direction of the current. The theory that the streaks were composed of concentrated sulphuric acid seemed open to great objections; for we know that concentration takes place at the anode in dilute sulphuric acid without a reversal of the current, and also does not depend on the use of palladium electrodes; but the streaks are, as is shown above, only to be obtained after a reversal of the current, and are not to be obtained with platinum electrodes. It was therefore decided to test, as far as practicable, other possible combinations, such as hydroxyl (the streaks cannot be composed of water, for they are denser than the surrounding electrolyte), a salt of palladium, &c.

#### *Testing for Hydroxyl.*

The palladium electrodes used were rectangular strips, about 2 centim. long and .3 centim. broad, placed vertically in 10-

per-cent. sulphuric acid. For the purpose of collecting the streaks a capillary glass tube had one end blown out into the shape of a small funnel, and was bent so that it could be lowered into the electrolyte with the finger pressed on the upper end, which projected from the cell, until the funnel part was immediately below the electrode. After a reversal of the current the funnel part was placed in this way under the anode, from which the streaks were flowing. The finger was then removed from the top end of the tube, and the funnel acted as a reservoir for the denser liquid flowing from the anode. As soon as gas appeared at the anode the finger was replaced on the top end of the tube, the latter being then removed and its contents emptied into a test-tube. This proceeding was repeated many times (at least fifty), and the total liquid collected was tested for hydroxyl by the addition of a few drops of a solution of titanous acid in strong sulphuric acid. No trace of a coloration could be perceived, and thus the presence of hydroxyl was not proved\*.

\* The test, which is a delicate one, has been greatly employed by Richarz in his experiments on the mode of appearance of hydroxyl at the anode in sulphuric acid (*Wied. Ann.* xxxi. p. 912, 1887). He concludes that the appearance of hydroxyl at the anode is caused by the purely chemical decomposition of the "Ueberschwefelsäure" (Berthelot's  $\text{S}_2\text{O}_7 + \text{H}_2\text{O}$ ), which is first formed there. Traube, *Berichte der deut. chem. Gesell.* xviii. p. 3348 (1888) gives the equation for this decomposition as



Richarz confirms Berthelot's statement that no hydroxyl is formed at the platinum anode unless the strength of the acid is above 60 per cent. As we obtained the streaks in 10-per-cent. acid, the above fact lends additional strength to the view that the streaks are not hydroxyl. They might consist of  $\text{H}_2\text{S}_2\text{O}_8$  (Ueberschwefelsäure), which Berthelot has shown to be a minor secondary product formed during the electrolysis of sulphuric acid, whether dilute or strong (see *Ann. de Chim. et de Phys.* xiv. 1878, and xxi. 1880). The quantity so formed at a platinum anode is small, and would apparently be still smaller at a palladium anode charged with hydrogen if the following reasoning be accepted. Let us first consider the case of an anode uncharged with hydrogen. Assuming that the ion liberated at the anode is  $\text{SO}_4$ , the simplest hypothesis to account for the secondary reactions is that part of the  $\text{SO}_4$  combines directly with the  $\text{H}_2\text{SO}_4$  of the electrolyte to form  $\text{H}_2\text{S}_2\text{O}_8$ , whilst the remainder of the  $\text{SO}_4$  (probably by far the greater part) acts on the  $\text{H}_2\text{O}$  of the electrolyte, forming  $\text{H}_2\text{SO}_4$  and liberating O. Should, however, the anode be charged with H, the  $\text{SO}_4$  would have an additional tendency to combine directly with the H, forming  $\text{H}_2\text{SO}_4$ . This tendency would probably greatly exceed the other two, so that if sufficient occluded H be present, it would be expected that neither O would be liberated, nor  $\text{H}_2\text{S}_2\text{O}_8$  formed. It is known that there is no evolution of O under these conditions; and it would be interesting to estimate the relative amounts of  $\text{H}_2\text{S}_2\text{O}_8$  produced at a palladium anode when charged and when uncharged with H. The  $\text{H}_2\text{S}_2\text{O}_8$  could be quantitatively determined by the method used by Richarz (*loc. cit.* p. 917).

*Testing for Palladium Salts.*

A supply of the liquid having been obtained by a similar process to the above, a solution of potassium iodide was added to it, but no evidence of the presence of palladium was obtained.

*Streaks obtained in Phosphoric Acid.*

Similar streaks were obtained in solutions of pure orthophosphoric acid by adopting exactly the same procedure as with sulphuric acid.

*Streaks obtained in Caustic-Soda Solution.*

In this case also streaks were obtained descending from one of the electrodes, but the conditions necessary for their appearance are very different to those in the case of sulphuric and phosphoric acids. *No previous reversal of the current is necessary*, and the streaks descend from the kathode immediately the current is passed. The gas (hydrogen) formed at the kathode is at first absorbed, and the streaks are seen to descend as long as the absorption takes place; but, when gas begins to be evolved briskly, the streaks disappear. By very careful observation it is seen that the liquid of which the streaks are composed is still formed, but is carried up in the current of gas to the surface of the electrolyte, from which it rebounds, giving the electrolyte round the kathode the peculiar wavy appearance which accompanies the incomplete mixing of two liquids of different densities, such as two strengths of the same solution.

*Explanation of the Caustic-Soda Streaks.*

This experiment led us to infer that the streaks in the caustic-soda solution are composed of concentrated alkali, which is known to be formed at the kathode, and that the absence of evolution of gas from that electrode is a necessary condition for their appearance and steady downward flow. The part played by the palladium in the formation of the streaks is, according to this theory, that of absorbing the hydrogen, which otherwise, in escaping from the electrode, would carry the concentrated solution along with it to the surface and thus prevent the formation of the streaks.

*Explanation of Streaks in general.*

Since in acids concentration occurs at the anode, the descending streaks, if seen at all, should be seen at that electrode. But taking sulphuric acid as an instance, oxygen is given off at the anode, and the palladium cannot retain it sufficiently



to stop its evolution, and thus the concentrated acid will be carried up by the current of oxygen and escape observation. But if the electrode has been filled with hydrogen by previously serving as kathode, the evolution of oxygen on reversal of the current is prevented either by its direct union with the occluded hydrogen, or by the union of the ion  $\text{SO}_4$  with the hydrogen. No gas being evolved, the concentrated acid is able to flow downwards in streaks. This explanation is borne out by the experiments with sulphuric acid detailed above, in which it was seen that, on placing freshly-heated palladium electrodes in sulphuric acid, gas is immediately evolved at the anode; but if that electrode has been previously filled with hydrogen, the oxygen, on reversal of the current, does not appear for some time at the anode, and the streaks are visible during this period.

Concentration of the solution at one electrode is accompanied by a weakening of the solution at the other electrode. Therefore, as in acids the weakening takes place at the kathode (at which the gas is absorbed), we should expect to see streaks ascending from that electrode without a previous reversal of the current. On trying the experiment with palladium electrodes placed *horizontally* in dilute sulphuric acid, this supposition is found to be warranted. The reason why the streaks ascending from the kathode were not seen in our earlier experiments is, that the electrodes had been placed vertically with their top edges a little below the surface of the electrolyte, and so the weakened solution in ascending had naturally clung to the surface of the electrode and thus escaped detection.

The late Professor Christiani (in a work\* to which we shall refer more fully in a subsequent paper) gives three instances in which he observes streaks from the electrodes, but does not offer any explanation of their mode of formation. He cites the cases of zinc electrodes in concentrated zinc sulphate and copper electrodes in concentrated copper sulphate†. In these cases, without a previous reversal, he observed streaks descending from the anode and ascending from the kathode at the same time. These results are evidently in accordance with the theory proposed above: no gas is given off at either electrode, and so the concentrated solution is allowed to descend in streaks from the anode and ascend in streaks from the (horizontal) kathode.

\* "Ueber irreciproke Leitung elektrischer Ströme," 1876, R. Friedländer; Wied. *Elect.* ii. p. 727.

† Christiani, *loc. cit.* p. 100.

Another confirmation is given by the behaviour of potassium sulphate during electrolysis. After a current has been passed in the same direction through potassium sulphate, it is found, on testing with litmus paper or by cautiously adding litmus solution, that the bottom strata of liquid have become alkaline and the top strata acid, the solution being originally neutral. It follows from this that the alkaline solution produced at the kathode is denser than the electrolyte, which itself is denser than the acid solution produced at the anode. Therefore, according to theory, the streaks, if seen at all, should descend from the kathode and ascend from the anode. This is fully borne out by experiment. Starting with freshly-heated palladium electrodes, on putting on the current we immediately see the streaks descending from the kathode; and after a reversal of the current other streaks may simultaneously be detected rising from the anode.

The best conditions for seeing the streaks seem to be :—

- (1) a horizontal electrode of small surface\*,
- (2) a strong current,
- (3) no evolution of gas.

The conditions (2) and (3) are incompatible with the use of platinum electrodes in dilute sulphuric acid : it is quite possible, however, that even in this case matters might be so adjusted that some trace of the formation of the streaks could be detected. In the case of platinum electrodes in solutions of sulphates of metals which do not act readily on water, such as zinc, copper, iron, &c., the metal will be deposited and no gas evolved at the kathode, and thus streaks of the weakened solution can be seen at that electrode†.

A direct proof that the streaks were formed of concentrated acid might be thought possible, in the case of sulphuric acid, on consideration of the following circumstances :—

- (1) When strong sulphuric acid is dropped into dilute sulphuric acid, the strong acid sinks to the bottom of the containing vessel and remains undiffused for a considerable period.
- (2) If collecting-vessels are placed under the electrodes and an arrangement fitted up by which the current may be periodically reversed, say every minute, the streaks will be formed at the electrodes and will stream into the collecting-vessels.

\* A vertical electrode may be used with advantage if it is well below the surface of the electrolyte and if it is suspended by a horizontal wire, so that the view of the electrolyte above and below it is not interrupted. An angular electrode is very convenient, as the liquid flows most readily from points.

† Christiani noted the streaks with platinum electrodes in ferrous sulphate.

(3) If we now titrate with standard caustic-soda solution equal volumes of the liquid in the collecting-vessels and of the main bulk of the electrolyte, we ought to find that the former is the more acid.

It may, however, be noticed that, assuming the streaks were not concentrated acid, we should collect by this arrangement, not only the streaks, but also some of the concentrated acid which is formed at the anode. Thus, even if the acid in the collecting-vessels did become stronger, it would scarcely prove that the streaks were the cause of this\*.

From the experiments detailed above, though direct proofs are apparently not available, we may draw the following general conclusions:—That when gas is not evolved at an electrode, streaks are formed there. These are due either to a concentration or weakening of the electrolyte, as in the case of solutions of acids and alkalies and some salts (such as zinc sulphate); or, in the case of other salts (such as potassium sulphate), to a chemical change in the electrolyte, yielding at one pole alkali and at the other pole acid, producing solutions of different density to the electrolyte. When gas is not evolved at an electrode these changes are still produced, but their effect in producing streaks is destroyed by the evolution of the gas.

In our next paper we hope to describe some experiments in which these effects become of great importance in changing the resistance of the electrolyte.

---

XXXVI. *On a Modified Water-dropping Influence-machine.*  
By Professor SILVANUS P. THOMPSON, D.Sc.†

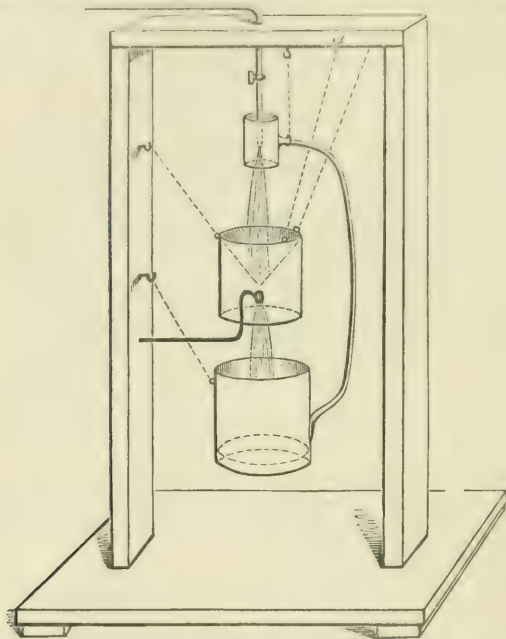
THE ordinary form of water-dropping influence-machine, as devised by Sir William Thomson‡, possesses some inconveniences: it requires a double jet of water and special arrangements for high insulation. A simpler form, requiring but one water-jet and mere silk strings (well paraffined) as insulators, has been found by the author to give far less trouble, and to work well for lecture demonstrations.

\* Some observations have been made by this method, but the results have not been very definite. We have designed an automatic commutator which will reverse the current at intervals of 15, 30, or 60 seconds. This arrangement will enable us to collect more easily the substance forming the streaks.

† Communicated by the Physical Society: read January 28, 1888.

‡ Proc. Roy. Soc. June 20, 1867; and Reprint of Papers on Electrostatics, p. 321.

From a wooden frame are hung by silk strings three simple metal vessels, the highest and lowest being rigidly connected together with a stiff metal wire. The highest is a small cylinder open at both ends : the lowest is an open pot which receives the water. The intermediate vessel is open at



the bottom ; and is provided at the top with a funnel, the upper rim of which is soldered inside the lip of the cylinder, and its depth such that its central aperture is about at the middle of the cylinder. An insulated wire, recurved as shown, is carried up clear under the funnel in the middle vessel, and should touch the drops as they fall below the aperture. The water-jet, which must have a fine orifice, is inserted about half-way into the uppermost vessel. A single point of water will suffice to gather a plentiful charge. To watch the process of charging two gold-leaf electroscopes may be connected respectively to the middle and to the lowest vessels.

The same arrangement will answer for sand-dropping if a second, uninsulated funnel to contain the sand be provided above the topmost cylinder, and arranged with its lower end entering into the cylinder, so that the jet of sand breaks away from the orifice at the proper height. As dry sand is a very bad conductor, the apparatus is found to work with



greater certainty if the sand is previously agitated with finely powdered plumbago, or with some sufficiently adherent metallic powder, such as the finer qualities of Bessemer bronze.

XXXVII. *The Effect of Magnetization on the Thermoelectrical and other Physical Properties of Bismuth.* By HERBERT TOMLINSON, B.A.\*

IN a paper read before the Royal Society on January 26, 1882†, the author has given an account of an experiment relating to the effect of longitudinal magnetization on the electrical resistance of bismuth. The subject has since been taken up much more fully by Righi‡, Leduc§, Hurion||, and Albert v. Ettingshausen and Walther Nernst¶. In the author's paper quoted above are also described experiments on the effects of magnetization on the electrical resistance of iron, steel, nickel, and cobalt, the results of which are summarized in the following table:—

TABLE I.

Metal.	Condition.	Increase of resistance, per unit produced by a C.G.S. unit of magnetizing force, $\frac{\Delta r}{rM_f}$	Magnetic susceptibility. $\kappa^{**}$ .	$\frac{\Delta r}{rM_f} / \kappa^{**}$ .
Iron .....	Annealed.	$2335 \times 10^{-8}$ .	30	$0.8 \times 10^{-6}$
Steel .....	Annealed.	1500 "		
Steel .....	Unannealed.	1137 "		
Steel .....	Very hard.	70 "		
Nickel .....	Annealed.	8070 "	8.8	9.2 "
Nickel .....	Unannealed.	4343 "		
Cobalt. ...	Unannealed.	638 "	4.4	1.4 "
Bismuth ...	Unannealed.	21 "	—0.00014††	1500 "
Copper ...	Annealed.	0†† "		

\* Communicated by the Physical Society: read January 28, 1883.

† "The Influence of Stress and Strain on the Physical Properties of Matter," *Phil. Trans.* vol. clxiv. (1883, part 1). Abstract, *Proc. Roy. Soc.* No. 218 (1882).

‡ *Acc. R. dei Lincei*, 1883, 1884.

§ *Bull. de la Soc. française de Phys.* 1884.

|| *Comptes Rendus*, 1884, 1885.

¶ *Sitzb. der kais. Akad. der Wissensch.* 1887.

\*\* The values of  $\kappa$  were determined for the same magnetizing forces as those used for producing alteration of resistance.

†† Taken from Von Ettingshausen's determinations (*Wien. Ber.* 1882).

‡‡ No change amounting to  $\frac{1}{4000000}$  could be detected with a magnetizing force of 90 C.G.S. units.

The data in this table refer to the effects of *temporary* magnetization; and in the case of iron, steel, and nickel, represent only verifications and extensions of the labours of previous observers. Abraham, Edlund, Mousson, and Wartmann all made search for magnetic alteration of the resistance of iron. W. Thomson\* seems, however, to have been the first to arrive at any definite result. He found the resistance to be increased along the lines of magnetization and decreased across them. W. Thomson has been followed by Beetz†, Tomlinson‡, Chwolson§, Auerbach||, and De Lucchi¶. These have all confirmed the results of Thomson so far as longitudinal magnetization is concerned; but Beetz failed to obtain anything but negative results with transverse magnetization, and attributed the decrease of resistance observed by Thomson to mere mechanical pull. The author has, however, pointed out\*\* the improbability of this last supposition. W. Thomson had also previously proved†† that the electrical resistance of nickel is increased to a greater extent than that of iron by longitudinal magnetization; whilst Faé‡‡ has recently verified the author's result concerning cobalt. Lastly, Goldhammer§§ has published a comparative study of the three paramagnetic metals—iron, cobalt, and nickel, and of the three diamagnetic metals—bismuth, antimony, and tellurium|||.

There are several points in the table given above to which it is desirable to direct attention. In the first place the resistance of all the metals is *increased* by longitudinal magnetization. In the second it by no means always follows that the metals which possess the greatest magnetic susceptibility are those which are most affected in their conductivity by a given amount of magnetizing-stress. We see, for instance, from the third and fourth columns, that whilst the value of  $\frac{\Delta r}{rM_f}$  for nickel is twice that for iron, the magnetic susceptibility of iron is between three and four times as great as that of nickel. It is, however, when we come to consider the fifth column that we meet with the most remarkable differences in the

\* "Electrodynamic Qualities of Metals," Phil. Trans. 1856, Part iv.

† Pogg. Ann. vol. cxxviii. (1886).

‡ Proc. Roy. Soc. vol. xxiii. (1875). Also *loc. cit.*

§ Carl's Rep. vol. xiii. (1877). || Phil. Mag. vol. viii. (1879).

¶ Atti del R. Ist. Veneto, viii. (1882).

\*\* *Loc. cit.* pp. 165, 166.

†† Proc. Roy. Soc. vol. viii. (1857). ‡‡ Atti del R. Ist. Veneto, 1887.

§§ Wied. Ann. xxxi. (1887).

||| See also a memoir, entitled "An Experimental Study of the Influence of Magnetism and Temperature on the Electrical Resistance of Bismuth and its Alloys with Lead and Tin," by Edmond von Aubel. Phil. Mag. p. 191 of the present volume.

effects of magnetization. The numbers in this column represent the increase of resistance per unit which would be produced in each of the metals by magnetizing them to such an extent that each cubic centimetre would possess unit magnetic moment. The effect of the magnetization in this case would be nearly twice as great for cobalt as for iron, twelve times as great for nickel as for iron, and, speaking very roughly, *two thousand* times as great for bismuth as for iron. Startling as this last result is, it sinks into insignificance when contrasted with the results obtained by other observers. The effect of magnetization on the electrical resistance of bismuth is largely influenced, amongst other things, by the amount of impurity in the metal and the magnitude of the magnetizing force. Thus, from Ettingshausen's researches, from which Table II. has been compiled, we learn that the value of  $\frac{\Delta r}{rM_f}$  for pure bismuth and for very large magnetizing forces\* may become nearly 200 times as great as

TABLE II.  
Transverse Magnetization.

Magnetizing force, in C.G.S. units.	Increase of resistance per unit produced in pure bismuth by a C.G.S. unit of magnetizing force, $\frac{\Delta r}{rM_f}$	Ditto in bismuth alloyed with one per cent. of tin.
1600	$1610 \times 10^{-8}$	$660 \times 10^{-8}$
3160	2490 "	
5880	3350 "	
8410	3660 "	
10470	3840 "	
11200	3890 "	

that observed by the author†, with the comparatively low magnetizing force of 130 C.G.S. units; whilst the introduction of one per cent. of tin diminishes the value to *one sixth*.

\* The bismuth was acted upon by a transverse magnetizing force, which, however, has been proved to *increase* the resistance, though to a greater extent than a longitudinally magnetizing force.

† The specimen of bismuth used by the author has been, through the kindness of Professor J. M. Thomson, analyzed at the chemical laboratory at King's College, London; it contains 95.48 per cent. of bismuth.

Provided that the susceptibility of bismuth be the same for very high magnetizing forces as for low ones, it would follow that if a bar of pure bismuth could be magnetized to unit intensity\* its resistance would be nearly trebled, whilst the increase of resistance per unit would be at least 300,000 times as great as the corresponding change of resistance in iron. Considerations such as the above render it difficult to believe that what is observed in bismuth in such experiments as these is a real change in the specific resistance of the metal; and even with iron, nickel, and cobalt there seems to be evidence that the *whole* of the observed change produced by magnetization is not produced by mere rotation of the molecules. In the author's experiments on iron and nickel he found that the increase of resistance could be very closely represented by the formula

$$\frac{\Delta r}{r} = aM_f + bM :$$

where  $\frac{\Delta r}{r}$  denotes the increase of resistance per unit, and  $M_f$  and  $M_i$  are the magnetizing force and the magnetic intensity respectively. From this formula it follows that, even if the magnetizing force were so high that the ratio of increase of intensity to increase of force was extremely small, the resistance would nevertheless go on increasing very perceptibly indeed with the force. The values of the coefficient  $b$  were not very different in the two metals nickel and iron; but the coefficient  $a$  in nickel was about five times as great as in iron, and was nearly double the coefficient  $b$ . It would be a matter of considerable interest to ascertain whether it would really happen that, when very great magnetizing forces were employed, the resistance of nickel would go on increasing very perceptibly with the force†.

If, however, on the one hand there is a difficulty in conceiving how magnetization can so largely affect the true specific resistance of bismuth, there is also an equal difficulty in accounting otherwise for what is observed. Hall's phenomenon cannot certainly be credited in any of the experiments which have yet been made with anything but a small fraction of the whole observed effect. It is true that bismuth has been found by Righi and others to have a very large rotational coefficient as compared with iron, nickel, or cobalt; but Ettingshausen and Nernst have shown‡ that whilst, with

\* This would require a magnetizing force of 71,000 C.G.S. units.

† The magnitudes of the forces used by the author never exceeded 200 C.G.S. units.

‡ *Loc. cit.*



bismuth, antimony, and tellurium, the increase per unit of resistance produced by a magnetizing force of 7660 C.G.S. units is 0.20, 0.006, and 0.0014 respectively, the corresponding values of the rotatory power are  $-4.7$ ,  $+0.18$ , and  $+790$ .

Neither, again, can the change of dimensions produced by magnetization in any of the metals be accountable for the increase of resistance. For though, curiously enough, loading an iron wire increases the resistance, and magnetizing it longitudinally increases the length, whilst loading a nickel wire\* *decreases* the resistance and magnetization *decreases* the length, yet, according to Joule and others, when an iron wire is loaded to a certain extent longitudinal magnetization begins to decrease the length; whereas the author has shown that, even when iron is loaded nearly to breaking, longitudinal magnetization always produces increase of resistance. Besides, the changes of dimensions in nickel, iron, and bismuth produced by magnetization are far too small†. Here again, however, it should be noticed that both the decrease of length produced by magnetization and the decrease of resistance produced by loading a nickel wire are considerably greater than the corresponding increase in the case of iron.

Whatever it is that causes magnetization to produce so large an effect on the electrical conductivity of bismuth, causes it to produce also a large effect on some of the other physical properties. The thermal conductivity of bismuth is, according to Leduc‡ and Righi§, decreased both by longitudinal and transverse magnetization by an amount which is about equal to the amount of decrease produced in the electrical conductivity; and though it would appear, from Ettingshausen's observations||, that the decrease of thermal conductivity is decidedly less than the decrease of electrical conductivity, yet even this observer makes the former comparable with the latter; and we shall now see that the thermoelectrical properties of bismuth are quite as largely affected by magnetization as either the thermal or the electrical conductivity.

### *The Thermoelectrical Properties of Bismuth.*

Sir W. Thomson has shown¶ that iron longitudinally magnetized is negative, and transversely magnetized positive, to iron unmagnetized. Barus and Strouhal\*\* have also investi-

\* See Phil. Trans. 1883.

† Prof. Barrett failed to detect any change produced in the dimensions of bismuth by magnetization.

‡ *Comptes Rendus*, 1887.

§ *Ibid.*

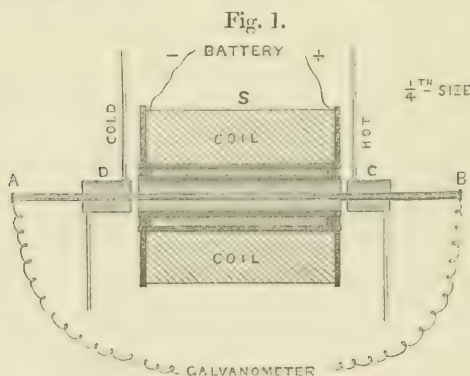
|| *Annalen der Physik und Chemie*, Band xxxiii. (1888).

¶ "Electrodynamical Qualities of Metals," Phil. Trans. Part iv. 1856.

\*\* Bulletin of the U. S. Geological Survey, No. 14 (1885).

gated with great completeness the influence of magnetization on the thermoelectrical properties of steel of different tempers. Finally, Ewing has entered very fully\* into the changes effected by longitudinal magnetization in iron when under different amounts of longitudinal stress. Thomson has also shown† that nickel is rendered by longitudinal magnetization thermoelectrically positive to unmagnetized nickel; whilst the author has found‡ cobalt when under longitudinal magnetization to be negative to the unmagnetized metal.

The experiment now about to be described was made nearly at the same period as the experiment on the effect of magnetization on the electrical resistance of bismuth and with the same bar. This bar was 25 centim. long and 0·33 centim. in diameter; it was placed in the axis of a magnetizing solenoid, S, specially constructed to avoid imparting heat to the magnetized core§; a preliminary examination proved that there was certainly no error arising from this cause. The arrangements are sufficiently shown in fig. 1, where S is the



solenoid and AB the bar. The bar was encircled by two little air-chambers, C and D, through one of which steam was passed, and through the other water at a temperature of about 16° C. The ends A and B were connected by copper wires with a very sensitive Thomson's reflecting-galvanometer, and were well buried in sawdust.

Since a bar of bismuth can never be obtained in a perfectly homogeneous condition throughout its whole length, there was a considerable thermoelectrical current already in existence

\* Phil. Trans. Part ii. 1886.

† *Loc. cit.*

‡ Proc. Roy. Soc. No. 241 (1885).

§ For a description of this solenoid see Phil. Trans. 1883, *loc. cit.*

before magnetization and the spot of light was driven off the scale. The light could be again brought on the scale by putting the adjusting-magnet low down; but this of course materially diminished the sensibility of the galvanometer, and as the effect to be looked for was likely to be very small this was not desirable. Accordingly the following plan was adopted:—There were two sets of needles in the galvanometer, connected with each other and the mirror; round one set was wound a coil of about 6000 ohms resistance, and round the other a coil of between 7 and 8 ohms resistance; the extremities of these coils were attached to separate terminals, and the latter coil was employed to measure the thermoelectrical effect of magnetization. The thermoelectrical current due to want of homogeneousness in the bismuth was balanced by the current from a Daniell cell sent through the other coil, which was shunted, and through a very large resistance: by altering this resistance the spot of light could easily be brought again to the middle of the scale. Some two hours were allowed to elapse, the steam during the whole of this time passing through the air-chamber, C, and the cold water through the air-chamber, D, after which the spot of light remained steady. The solenoid, S, was now actuated by a current from six Grove cells, and a deflection ensued indicating a current *from unmagnetized to magnetized bismuth through the hot junction*. The current through the solenoid was then stopped, and the spot of light returned sensibly to its old position. The observation was repeated ten times, and then the current through the solenoid being reversed, ten other observations were made, after which the current was again reversed. The readings had to be corrected for a slight direct action of the magnetizing solenoid on the needles of the galvanometer.

The deflection due to the thermoelectrical current between magnetized and unmagnetized bismuth was very small; but, so far as could be made out, it was the same for both directions of the magnetizing current. The mean of the readings gave a deflection of 3·5 divisions of the scale; and, by independent observation with a Daniell cell, it had been ascertained that a deflection of 1 division of the scale would, under the conditions of the experiment, indicate an E.M.F. of 0·143 microvolt. Consequently the E.M.F. produced by temperatures of 100° C. and 16° C. at the two junctions of magnetized and unmagnetized bismuth would be  $0·143 \times 3·5$  microvolts =  $\frac{1}{2}$  microvolt. The magnetizing force was 226 C.G.S. units; so that the E.M.F. for unit magnetizing force would be ·0022 microvolt, or ·22 C.G.S. units of E.M.F. If

we divide the last number by  $14 \times 10^{-6}$ , the magnetic susceptibility, we shall obtain the E.M.F. which would be produced by magnetizing the bismuth to unit intensity; this is 15714 C.G.S. units. Now, according to Ewing, the E.M.F. produced in a certain specimen of soft iron wire by a magnetic intensity of 160 C.G.S. units was 6 microvolts, when the junctions of the magnetized and unmagnetized wires were at  $100^{\circ}$  C. and  $16^{\circ}$  C. respectively. Accordingly the E.M.F. produced by unit magnetic intensity would be 3.75 C.G.S. From this it is evident that, for a given intensity of magnetization, bismuth has its thermoelectrical properties altered by longitudinal magnetization 4000 times as much as iron. We see, then, that the relative changes produced by magnetization in the thermoelectrical properties of bismuth and iron are comparable with the changes wrought in the electrical and thermal conductivities of these metals.

Grimaldi has already published researches\* on the effect of both transverse and longitudinal magnetization on the thermoelectrical properties of bismuth, and finds that magnetization in either of these two directions makes the bismuth of commerce thermoelectrically negative to unmagnetized bismuth. He also finds that the thermoelectrical E.M.F. of a *pure* bismuth and copper couple is increased by both longitudinal and transverse magnetization. Now according to Ettingshausen†, who also quotes Rollmann‡, *pure* bismuth is thermoelectrically *negative* to copper, whilst commercial bismuth is positive to copper. The following table is taken from Ettingshausen's memoir:—

TABLE III.—Bismuth alloyed with different amounts of Tin.

Potential difference, in C.G.S. units, for $1^{\circ}$ C. between a bismuth and copper couple with one junction at $20^{\circ}$ C. and the other at $0^{\circ}$ C.	Number of parts, by weight, of pure bismuth in 100.
—6500	100.00
+ 280	99.05
+1950	98.54
+3910	93.86
+3390	86.90

\* *R. Acc. dei Lincei*, vol. iii. Also memoir, entitled *Influenza del Magnetismo sulle Proprietà Termoelettriche del Bismuto* (Palermo, 1887); *Journal de Physique*, Dec. 1887, p. 569.

† *Sitzb. der kais. Akad. der Wissensch.* 1887.

‡ Pogg. *Ann.* lxxxiii., lxxxiv., lxxxix.



Consequently it would seem to follow, from Grimaldi's experiments, that *both pure and commercial bismuth* are rendered by magnetization negative to the unmagnetized metal, *i. e.* the thermoelectrical current would flow from unmagnetized bismuth to magnetized bismuth through the hot junction.

Grimaldi shows that, in the following respects, the effect of magnetization on the thermoelectrical properties of bismuth resembles the effect of magnetization on the electrical conductivity:—

(1) The amounts of the two effects are comparable with each other.

(2) Transverse magnetization produces a greater effect than longitudinal magnetization.

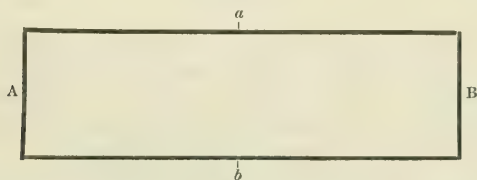
(3) Rise of temperature\* diminishes the effect.

(4) The effect increases in greater proportion than the magnetizing force.

It is impossible for the author to compare his own results with those of Grimaldi; but it would seem from the above that, by using high magnetizing forces and lower temperatures at the junctions, the effect of magnetization on the thermoelectrical properties of bismuth might well be found to be some three or four hundred thousand times the effect in iron, supposing both metals to be magnetized to unit intensity.

This being the case, it is difficult to believe that the alteration due to magnetization is a real alteration of the thermoelectrical power of the metal. But, again, how are we to account for it. According to Ettingshausen†, when a plate of bismuth, A B (fig. 2), is arranged, as for experiments on

Fig. 2.



Hall's phenomenon, with its plane parallel to the flat faces of the pole-pieces of an electromagnet and perpendicular to the lines of force, whilst a current of electricity is conducted longitudinally through the plate, the excitation of the electromagnet produces a difference of temperature at the two points *a* and *b*; whilst, on the contrary, if a current of heat be conducted

\* At least as far as 100° C.

† *Annalen der Physik und Chemie* Band xxxi. (1887).

through the plate instead of the electrical current, there will be produced by the action of the electromagnet a difference of potential at the two points\* *a* and *b*. Further, besides the difference of potential at *a* and *b*, which is designated the transverse "thermomagnetic effect," there will be a difference of potential at *A* and *B* called the longitudinal thermomagnetic effect; and this last, says Ettingshausen, will account for the apparent effect of magnetization on the thermoelectrical properties of bismuth. Both Grimaldi† and Leduc‡, however, are of opinion that the apparent longitudinal thermomagnetic effect is produced by decrease of thermal conductivity and thermoelectrical power.

The alteration of dimensions produced by magnetization can as little account for the change in the thermoelectrical properties of metals as for the increase of resistance; for, besides the minuteness of the alteration of dimensions, in some cases the effect of loading on the thermoelectrical power is in the same direction, and in others in the opposite direction, to the effect of longitudinal magnetization, as will be seen from Table IV.

TABLE IV.

Metal.	Under longitudinal traction§.	Under longitudinal magnetization  .
Iron .....	—	—
Nickel .....	+	+
Cobalt .....	+	—
Bismuth .....	+¶	—

There is, however, this resemblance between the effects of magnetization on the thermoelectrical properties of iron and on its dimensions. When an iron wire is loaded beyond a certain limit, magnetization begins to produce decrease of length and increase of thermoelectrical power\*\*.

In conclusion, when we contrast the small magnetic susceptibility of bismuth with the large value of its rotational coefficient, and with the large decrease which can be produced

\* Ettingshausen and Nernst, *Wied. Ann.* xxix. (1886).

† *Nuovo Cimento*, ser. 3, vol. xxii. (1887).

‡ *Comptes Rendus*, 1887.

§ A plus sign shows that the stretched metal is positive to unstretched.

|| A plus sign shows that the magnetized metal is positive to unmagnetized.

¶ Righi, *R. Acc. dei Lincei, Transunti* (1884).

\*\* Cf. Ewing, *loc. cit.*

both by transverse and longitudinal magnetization in the thermal conductivity, the electrical conductivity, and the thermoelectrical power of the metal, we must be driven to the conclusion that magnetism in all metals exerts two distinct influences; one by rotation of the molecules about their axes, the other in some way which is not yet understood. In such metals as iron, and to a less extent in cobalt and nickel, the first of these influences probably plays a not unimportant part; but in such metals as bismuth, antimony, and tellurium, the second must entirely predominate.

XXXVIII. *On a Method of Determining the Difference between the Phase of two Harmonic Currents of Electricity having the same Period.* By THOMAS H. BLAKESLEY, M.A.\*

IT has been brought to my notice by both English and Foreign journals connected with science that a method of determining the difference in phase of two Harmonic Currents of Electricity having the same period, which I invented and published some years ago, forms an important part of the subject matter of a paper communicated to the Royal Academy of Sciences of Turin, second series, vol. xxxviii., by Signor Galileo Ferraris, in which that philosopher lays claim to the invention above mentioned, producing it as original, with no sign of acknowledgment that the method has before been made public.

The method consists in employing the two coils of an electric dynamometer in a peculiar way. When an harmonic current is sent through the coils of such a dynamometer in series, its reading will measure the quantity  $\frac{I^2}{2}$ , where  $I$  is the maximum value of such a current. In this way we may successively determine  $\frac{I_1^2}{2}$  and  $\frac{I_2^2}{2}$ , where the subscripts refer to two currents having the same period.

But if we place one of the coils in one circuit and the other in the second circuit, the reading of the instrument will measure  $\frac{I_1 I_2}{2} \cos \theta$ , where  $\theta$  is the angle representing the phase-difference of the two currents, to which the name "*décalage*" has, I have no doubt with great propriety, been accorded by M. Hospitalier.

It is clear that from the three readings we can deduce the

\* Communicated by the Physical Society: read March 10, 1888.

angle of phase-difference. Thus,  $\alpha_1, \alpha_2, \alpha_3$  being the three readings, we have :—

$$\alpha_1 \propto \frac{I_1^2}{2},$$

$$\alpha_2 \propto \frac{I_2^2}{2},$$

$$\alpha_3 \propto \frac{I_1 I_2}{2} \cos \theta ;$$

$$\therefore \cos^2 \theta = \frac{\alpha_3^2}{\alpha_1 \alpha_2}.$$

Here I have supposed that the same instrument is used successively; and it is hardly necessary to point out that if we have three exactly similar dynamometers, or if we have three dynamometers of which the relative values of the constants are known, we can deduce  $\theta$  by means of three simultaneous observations.

The real importance of the determination of the *décalage* rests in the means it affords us of determining the causes of its existence; among which I may here mention Coefficients of Induction (self or mutual), Capacity of Condensers, and hysteresis or the waste of energy involved in the reversal of electromagnetic momentum, all of which present useful fields for inquiry capable of investigation by means of this application of the dynamometer, a few of which I have myself traversed.

I wish to point out that this method and its importance (not yet fully appreciated) were published by me in the 'Electrician' newspaper of October 2, 1885; and the entire series of papers, of which this formed one, was republished in book form at the end of the same year. This was before I had the honour of membership in the Physical Society, or doubtless I should have then brought the subject more immediately under your notice; but upon my being elected a member I lost no time in presenting a copy to this Society, and those who care to examine this question will find therein a chapter devoted to this very employment of the dynamometer to determine Coefficients of Self- and Mutual-Induction and the Capacity of Condensers.

March 1888.



XXXXI. *On Valency, Validity, and Residual Affinity.* By  
J. F. HEYES, M.A., F.C.S., F.R.G.S., of Magdalen College,  
Oxford.\*

THE timely paper by Professor Armstrong, entitled "A Note on Valency, especially as defined by Helmholtz,"† induces me to publish my paper on the tetravalency of oxygen, and to offer some remarks on his paper in the hope that both, so happily coincident and containing independent views and suggestions, will do something to bring forward the consideration of the problems of Valency which are so essentially characteristic of the present state of *Chemical Philosophy*. Raw material abounds. It is now surely more essential for chemists to study the inner mechanism, so to speak, of the chemical molecule both in the interrelationships of the parts and the forces concerned than to discover, say, more oxides of manganese when we have already too many. Especially would I venture to direct attention to the paradox which now confronts us in the contrast existing, on the one hand, between so many of the complex organic molecules whose "constitution" we know, or at any rate whose inter-atomic relationships are regular and certain, even to a degree of prediction within remarkable limits, and, on the other, between so many simple inorganic molecules. Why, in fact, should "the constitution" of complex  $C_xH_yO_z$  or  $C_pH_qO_rN_s$  be known, and simple  $N_xO_y$  remain a puzzle, which tends to make many chemists write the old unitary formula in despair and betake themselves to the seductive studies of molecular physics?

In my previous paper ‡ some probable causes of confusion have been discussed which have led to the different views held on valency, but the problem is one for the immediate future. Prof. Armstrong draws our attention to the electric-charge theory of valency—using that term, it is presumed, in the sense of a *property* of the atom which used to be called atomicity. My paper suggested that valency was primarily a *number* or numbers, the measure of something whose real character and denomination science is still seeking, but nevertheless a something towards whose perception electrical, thermal, and atomic mass ratio constants are rapidly converging. The number and the thing itself should not be confounded. But our fundamental standpoint in valency is that

\* Communicated by the Author.

† Phil. Mag. pp. 21–30 of the present volume.

‡ Phil. Mag. pp. 221–237 of the present volume.

of integers and not decimal residues\*. It is only in respect to our standard reference gas-molecule  $\text{HCl}$ , where hydrogen and chlorine are said to be monovalent, that the valencies of oxygen and nitrogen atoms in the comparable molecules can be said to be divalent and trivalent, *i.e.* dyads and triads†. In face of the enormous difficulties presented by "molecular compounds," we are apt to forget that all chemicals consist of chiefly heterogeneous and rarely of homogeneous compound molecules; and a rush has, as it were, been made to dispose of them by giving numerous valencies to either the non-metallic or the metallic atom supposed to dominate all the rest. Thus, Wurtz latterly assigned different valencies in different compounds to

Nitrogen . . .	1	2	3		5		
Halogens . . .	1		3	4	5		7
Uranium . . .		2	3		5		
Vanadium . . .			3	4	5		
Manganese . . .		2				6	7
Osmium &c. . .		2		4		6	8

Analogous high valencies have been suggested by Prof. Williamson and others for many rare metals of high atomic weights; but in this case the halogens were taken as monads. Both views have the same underlying principle. In the one case a Peter dominates the Pauls, in the other a Paul dominates the Peters. Both involve what I have pictured as the maypole theory. Oxygen atoms dance round nitrogen or

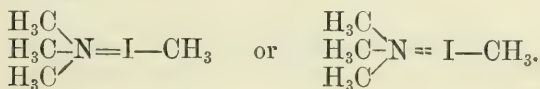
\* The logical alternative is, I find, well put in a remarkable paper on Atomic Valency by Prof. Pickering (Journ. Chem. Soc. 1886):—"The valency of hydrogen being 1, that of oxygen will be represented by some number slightly smaller than 2, say 1.98; a compound of these two elements will possess a *residual valency* of only 0.02, and will therefore be sufficiently saturated to be perfectly stable. It may be represented thus:  $(\text{H}_2\text{O}) + 0.02$ ." He therefore treats "valencies" as "not whole numbers," but "similar to most other physical constants, *e.g.* atomic weights, specific weights."

† A distinction should be drawn between this provisional definition of a trivalent element and the proposition advanced in Prof. Armstrong's paper (page 24) that "the gasifiable hydrogen compounds are the only compounds available for the direct determination of valency." Undoubtedly our present standard of reference is  $\text{HCl}$  as a comparable molecular formula; but it seems only fair, on the principle of reciprocity, not to leave the valency of metals to turn upon their few compounds or alloys with another gaseous metal hydrogen (see my former paper, page 225). The caution that, in the case of the halides,  $\text{XH}_n$  does not *necessarily* involve a valency of  $\text{X}^n$  is most important, and will, it is to be hoped, lead to some investigations into the existence and chemical habits of nuclei and ring-formulae of nitrogen or nitrogen and carbon, chlorine, iodine, oxygen, &c.

chlorine in the one case, democratic chlorine or oxygen atoms dance round a rare and monarchical metallic atom in the other. This is illustrated by Wurtz's arguments in support

of  $\text{NH}_4\text{Cl}$  as  $\begin{array}{c} \text{Cl} \\ | \\ \text{H} > (\text{N}) < \text{H} \\ | \\ \text{H} \end{array}$ ; and the new departure is indicated

by what Prof. Armstrong calls his first "apple of discord" when he disputes the evidence usually taken to justify the analogous domination of N in  $\text{Me}_4\text{NI}$ . I have been independently led to similar conclusions, and would write the constitutional formula as



The peck-lines are simply meant to show that the point for discussion is the valency or validity of the nitrogen and iodine atoms. Those who accept  $\text{NMe}_4\text{I}$  as not a molecular compound usually assume (1) that N is pentavalent, (2) that I is monovalent, (3) that there are *four* methyl groupings (or other R') directly and equally intimately associated with the nitrogen atom. The formula given simply asserts:—(1) That the chemical evidence favours a belief in the direct association of only three of the four methyls; (2) That in whatever sense the nitrogen atom is pentad not triad, in that same sense the iodine atom is interdependently triad not monad. If the nitrogen is pentavalent *here*, the iodine atom is also trivalent. But in order that the dispute may be fought out with the less risk of misunderstanding, my previous paper proposed (1) that the rarer or disputed *valency* may be referred to as *validity*, and (2) the latin prefixes and (3) roman numerals be exclusively used for this latter disputed number. Anything that tends to order and method in our present notation is at least worth consideration. Now nobody disputes that nitrogen is trivalent. Let us, then, in this and in other well-recognized cases keep uniformly to the greek prefix and symbolize a *trivalent* nitrogen atom as  $\text{N}'''$ , reserving the symbol  $\text{N}^v$  for the disputed *quinquivalent* nitrogen in such compounds as those under consideration whenever there is likely to be ambiguity. The first formula would thus assert that iodine was therein *trivalent*, the second that it is *tervalid*; but that whether validity is a property (electrical wholly or in part), a function of the atom of the same chemical intensity as that relatively suggested by the numbers 1, 2, 3, already defined, and known

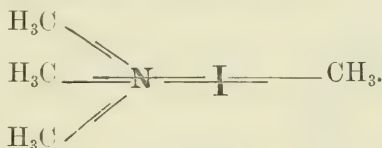
as valency or atomicity, is left open for investigation and discussion. It may also be useful to adopt the word *adivity*, long ago proposed by Prof. Odling as comprehending both *valency* and *validity*\*. Thus it is convenient to say that the adivity of oxygen is 2 or 4, that oxygen is usually divalent, but in some cases quadrivalent, if not actually tetravalent, leaving questions of molecular compounds not entirely closed on the chemical side, especially in the case of all  $\dot{X}.yH_2O$  "compounds." To take another example; in the case of the notorious CO molecule, if the oxygen atom is not there tetravalent the carbon atom is bivalent. The "molecular compounds" dispute does not here arise. On the other hand, in such formulæ as  $AgKI_2$  and  $AgI.KI$  sides are taken. On the former side we should write  $Ag-I=I-K$ ; whereas  $Ag-I \equiv I-K$  would suggest the controversy:—Is iodine here trivalent or is it tervalid in either some integral sense akin to atomicity or in the sense suggested by the working hypothesis of "residual affinity"?

There has undoubtedly been great ambiguity of expressions hitherto in this matter. Thus, whilst Wurtz admitted that " $NH_4Cl$  belongs to the type  $XX_5$ ," and held "generally and implicitly that the chlorine and the four atoms of hydrogen are united individually to the pentavalent nitrogen," he at the same time gave as two reasons why  $HCl$  and  $NH_3$  can unite:—(i.) "the atoms uniting are in possession of a *residue of affinity*;" (ii.) "the atoms of nitrogen can admit into their sphere of action a fourth atom of hydrogen and an atom of chlorine" (Atomic Theory, 1880, transl. p. 247). He went on to deprecate "creating and employing secondary hypotheses." "Why should we graft upon this atomic hypothesis a second, a *special attraction* which in a *completed combination* is exercised by one molecule upon another?" The italics are mine, and they should be compared with the views he expresses later on and the cases of *residual affinity* as Prof. Armstrong employs the phrase. In  $PCl_5$  Wurtz held that "two atoms of chlorine were *retained more loosely* than the other three"; but the retaining force was "most naturally" attributed to residence in the atoms themselves; the P atom in  $PX_5$  "can retain a *residue of energy* capable of fixing new atoms and of developing, if we may be allowed the expression, *supplementary atomicities*." These statements are of great

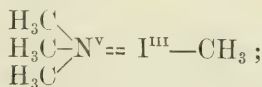
\* I note that Prof. Armstrong uses the word (p. 28) in "pentadivity of phosphorus," apparently in the more open sense of validity. As Prof. Odling used the word cautiously with reference to valency it may be convenient to retain it in the *inclusive* sense of  $P^{III}$  and  $P^V$ .



historic interest in the discussion, though scarcely consistent. Prof. Armstrong more logically inclines to the acceptance of only *one* number for valency concerned with what may be called chemical affinity of the first magnitude, and suggests that besides this affinity proper (say a trivalent atomic charge) there are residual affinities in play which probably account for molecular compounds and aggregates. Curiously enough he would, in many cases, thus place among "molecular compounds" many hitherto generally recognized "chemical molecules." Chemists will be loth to part with these. It is, as it were, taking our friends' dogs to the dog's home when the street dogs really demand our attention. He supposes that  $\text{NMe}_4\text{I}$  is "the molecular compound" (tetramethyl ammonium iodide) caused by the union of the two molecules  $\text{Me}_3\text{N}$  and  $\text{IMe}$  determined by the "residual affinities" of the N and I atoms represented thus :—



Whilst agreeing with the associations thus indicated, I propose that the N and I atoms be named in such cases "quinquivalent" and "tervalid" respectively,

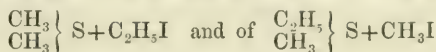


leaving it open for future research and discussion to determine the exact nature and limits of validity and valency, since we cannot at present commit ourselves to any partial theory, say either that of Helmholtz or of J. J. Thomson. Indeed we have also as chemists to keep an open mind on the sphere and vortex-ring hypotheses of an atom\*. It may, however,

\* Professor Armstrong says, "It may well be argued that we have no reason to assume that nitrogen is pentad in the ammonium compounds, or phosphorus pentad in the phosphonium compounds, or sulphur tetrad in the sulphine compounds" (p. 24). My proposal involves the idea that nitrogen is pentad *in whatever sense* chlorine is triad in  $\text{NH}_4\text{Cl}$  taken as  $\text{H}_3\text{N}=\text{Cl}-\text{H}$ , there being no reason to assume that the four atoms of hydrogen are directly associated with the nitrogen. Similarly for phosphonium compounds; but for other reasons (*e.g.* the evidence of the phosphinic acids, of  $\text{PO}(\text{C}_2\text{H}_5)_3$ ,  $\text{PO}(\text{C}_6\text{H}_5)_3$ , and other bodies) the pentavalency of phosphorus seems much better established than that of nitrogen. This observation tallies with the recognition of an "allotropic form" of

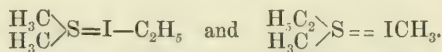
be convenient in electrolysis investigations, for instance, when there is no ambiguity, to use the phrase "residual affinity," and the appropriateness of the analogy to "residual charge" of a leyden jar is obvious. But it seems hard to place so many of the compounds mentioned in my former paper, and even  $\text{PCl}_5$  or  $\text{N}(\text{C}_2\text{H}_5)_4\text{OH}$ , outside the pale of honest "atomic" compounds—*i. e.* chemical molecules—and alongside the innumerable hydrates which molecular, or pro-

phosphorus not yet known in the case of nitrogen. The alleged cases of



giving distinct compounds (p. 27) of  $\text{S} \begin{Bmatrix} \text{C}_2\text{H}_5 \\ \text{CH}_3 \\ \text{CH}_3 \\ \text{I} \end{Bmatrix}$  is also interesting from

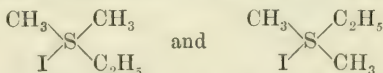
the possibility of iodine being trivalent or at least tervalid. Then there would be



Sulphur is at least tetravalent in whatever sense iodine is trivalent. But,

even assuming  $\text{S}^{\text{IV}} \begin{Bmatrix} \text{C}_2\text{H}_5 \\ \text{CH}_3 \\ \text{CH}_3 \\ \text{I} \end{Bmatrix}$  and the "equal value" of the "affinities" of

the sulphur atom, we might expect a case of *physical isomerism* in



where the methyl radicals hold *ortho* and *meta* "positions," as it were, even on the maypole hypothesis. Since writing I find striking statements by Professor Tilden on the trivalency of iodine written in 1876. On page 181 of his 'Introduction to Chemical Philosophy' (first edition) he

even gave a *closed-ring* formula for  $\text{KHgI}_3$  as  $\text{K}-\text{I} \begin{array}{c} \diagup \quad \diagdown \\ \text{I} \quad \text{I} \end{array} \text{Hg}$ , pointing out

that although "many chemists regard such compounds as formed by the union of entire molecules of the constituent salts combined together by some sort of adhesion differing from ordinary chemical affinity," yet "if they do they might naturally be expected to exhibit something of the appearance and external characters of their proximate constituents." He also considered the idea of "two extra units of combining power" for oxygen "not wholly preposterous," and sketched crystallized  $\text{ZnSO}_4 \cdot 6\text{OH}_2$  with "four bond" oxygen atoms throughout. This was, however, with a view to diminish the number of "molecular combinations," which in a sense it is now proposed to increase. There is much logic and tempting simplicity in the view that an atom is always *n*-valent, and that conflicting puzzles are "molecular," not "atomic," but this logical view seems intimately connected with the idea that the atom of N, for example, was born a manufactured article with three hooks, or bonds.

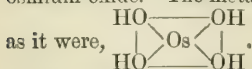
bably in many cases molar, physico-chemical experiments are forcing upon bewildered chemists. We must at any rate leave many of them in quarantine for some time to come. And, considering that the mortal chemist will always have to do almost all his work at the bottom of a varyingly moist aerial ocean, it is surely time that a protest was made against the claims for admission to the chemical record of so-called "new" bodies, which are simply more or less *moist* than the ordinary substance. Messrs. Carnelley and Walker have recently carefully \* adduced instances of these chemical impostures. It is quite refreshing to find among them an honest hydrate like  $\text{Ce}(\text{OH})_4$  which can stand  $600^\circ$  † ; but surely the line should be drawn at the temperature of hot water. In other cases of alleged "molecular compounds" some tangible *chemical* evidence should be required of their existence, or at least of the chemical homogeneity of the material or watered substance which is said to contain them. At the most, then, the so-called "silver hydrate" should be formulated

$\text{Ag}-\text{O}-\text{Ag}$   
as  $\begin{array}{c} \parallel \\ \text{HOH} \end{array}$  and *not* as  $\text{AgOH}$ . To use the latter and to

use  $\text{HCl}$  for ordinary "hydrochloric acid" is to strain out the gnat and swallow the camel. The fact probably is that it has become popular in preference to  $\text{Ag}_2\text{O} + \text{OH}_2$ , because of the frequent replacement of  $\text{Cl}$  by  $\text{OH}$  through its use in organic chemistry. Even in using the formula  $\text{HCl}$  conjointly with  $\text{NH}_4(\text{OH})$ , we are guilty of going against the weight of evidence, probably influenced, as Prof. Armstrong (rightly as it seems to me ‡) suspects, by the "ammonium" theory. Liquid hydrochloric "acid" does not dissolve a single metal, except perhaps aluminium. And it may be asked, Why should a liquid metallic chloride be expected to do so? The common equation  $\text{Zn} + 2\text{HCl} = \text{ZnCl}_2 + \text{H}_2$  is, of course, quite untrue. It is even more objectionable than the misleading sheet-anchor of so many textbooks:  $\text{Zn} + \text{SO}_4\text{H}_2 = \text{ZnSO}_4 + \text{H}_2$ . In "hydrochloric acid" the chlorine is probably tervalid and the

\* Journ. Chem. Soc. Jan. 1888, p. 59.

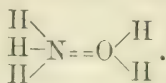
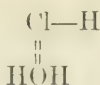
† This is probably connected with the quite *exceptional* volatility of osmium oxide. The metallic atom is possibly inside an oxygen nucleus,



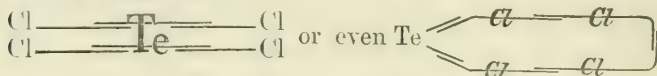
‡ This is instanced in the ingenious  $[\text{NH}_2(\text{Hg}_2)]'$  formulæ given in the case of mercur-ammonium and other difficult compounds. Of course

if  $\begin{array}{c} \text{Cl}=\text{Hg} \\ | \\ \text{Cl}=\text{Hg} \end{array}$  be admitted for calomel,  $\begin{array}{c} \text{Cl}=\text{Hg}'' \\ | \\ \text{NH}_2\text{Hg}'' \end{array}$  follows as a corollary.

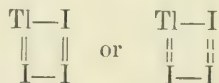
oxygen quadrivalent, just as in the case of "ammonium hydrate."



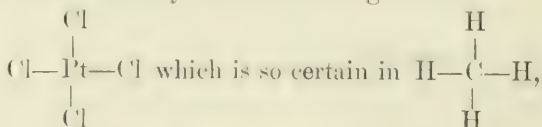
Probably Professor Armstrong would agree that chlorine as well as oxygen is here possessed of "residual affinity." But, adopting Thomsen's formula  $\text{H}_3\text{ClO}$ , the acid liquid would be  $\text{HCl}=\text{OH}_2$ . The more open view,  $\text{HCl}==\text{OH}_2$ , is then fairly warranted. It is this chemical unit which doubtless is active in effecting the hydrolysis of the alcoholic cyanides  $\text{R'CN}$ , which neither water alone nor liquefied hydrogen chloride,  $\text{HCl}$ , does effect. The suggestion that  $\text{TeCl}_4$  does not prove the tetravalency of tellurium is particularly interesting, since the suggestions of



are likely to lead to new developments in the discussion of the valency of the inorganic elements. The suggestion, on valency lines, had been made in my former paper, in the case for instance of  $\text{TI}_3$ , as



and the fact that two somewhat different lines of thought suggest these "closed-chain" molecules will, it is to be hoped, stimulate the discussion on valency. Undoubtedly the haloid compounds (except in the case of hydrocarbon derivatives) must be carefully examined before it can be claimed that a  $\text{XCl}_n$  "proves" the  $n$ -valent nature of  $\text{X}$ . This is especially the case with non-volatile chlorides, and is well exemplified by  $\text{PtCl}_4$ . In view of its chemical habitudes it is not easy for me to believe in the symmetrical arrangement



or even to be satisfied with

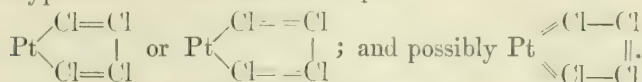


which leaves us with Hobson's choice.

Platinum tetrachloride has the properties of an asymmetrical chloride to a much greater degree than  $\text{PCl}_5$ . It seems likely



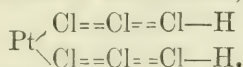
that the platinum is divalent, and yet that the peculiarity of the chlorine atoms—their residual affinity, validity, or even valency—enables  $\text{Cl}_2\text{Pt}$  to play the part of an unsaturated radical, but not because Pt is necessarily tetravalent. On this hypothesis I would write  $\text{PtCl}_4$  as



This indicates the chemical asymmetry of the two chlorine atoms. Either heat or free iodine removes the two remoter atoms (or, say, more accessible vortex-rings), giving in the latter case

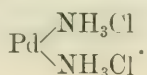
red crystals of  $\text{PtCl}_2\text{I}_2$ , *i. e.*  $\text{Pt} \begin{array}{l} \diagup \text{Cl}=\text{I} \\ \diagdown \text{Cl}=\text{I} \end{array}$ . Platinum is therefore

not octavalent in  $\text{H}_2\text{PtCl}_6$ , and probably not even tetravalent. The existence of the so-called molecular compound is simply due to the peculiarities of the chlorine, as suggested by

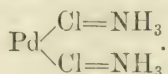


This is probably the type of the characteristic compounds of the platinum and palladium triplets, Rh, Ru, Pd and Ir, Os, Pt. The double chlorides are asymmetrical. The same idea applies to the characteristic  $\text{R}'_2\text{MF}_6$  of the Ti, Zr, Th and Si, Ge, Sn triplets. In cases where the chlorine atoms are symmetrical we often find an equal number of molecules associated by the mysterious dot (·). The type is  $\text{XCl}_n \cdot n\text{M}$  where M is  $=\text{ClK}$ ,  $=\text{NH}_3$ ,  $=\text{CO}$ ,  $\text{KCN} =$  and such like. Thus there is not only  $\text{TiCl}_3$  and  $\text{TiCl}_3$ , but also  $\text{TiCl}_3 \cdot 3\text{ClK}$ .

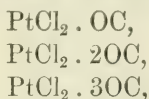
Validity is *inter alia* conditioned by temperature, and a temperature of  $200^\circ$  resolves either  $\text{H}_2\text{PtCl}_6$  or  $\text{PtCl}_4$  into  $\text{PtCl}_2$ . That Pt is divalent in  $\text{K}_2\text{Pt}(\text{CN})_4$  seems to be indicated by its formations from either  $\text{PtCl}_2$  or  $\text{PtCl}_4$ . Light will probably also be thrown upon the difficulties which beset attempts to give constitutional formulæ to the numerous isomeric or even metameric resultants between ammonia and chlorides. Their existence would appear to be due to this validity of the nitrogen and chlorine. For instance, of the two different substances formulated,  $\text{PdCl}_2\text{N}_2\text{H}_6$ , Pd being a well-defined dyad, the red one yields  $\text{NH}_3$  easily, and the yellow one gives no  $\text{NH}_3$  on treatment with  $\text{KHO}$ , but is the dichloride of a divalent radical,  $(\text{PdN}_2\text{H}_6)''$ , whose oxide and whose alkaline hydrate are known. In this yellow compound the chlorine is probably monovalent or the nitrogen pentavalent,



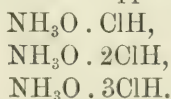
But in the red compound the chlorine is trivalent, or at least tervalid, and the nitrogen quinquivalent,



It will also be noticed that *both*  $\text{PtCl}_2$  and  $\text{PtCl}_4$  are, as the phrase goes, "unsaturated compounds." This could not well be the case if the valency of the platinum were altered in  $\text{PtCl}_4$ ; and they must therefore be due to the tervalidity of the chlorine, rather than the tetravalency, or to the residual affinity of the platinum. Thus,  $\text{PtCl}_2$  yields  $(\text{PtCl}_2\text{P})\text{Cl}_3$ ,  $\text{PtCl}_2\text{P}(\text{OH})_3$ , and  $\text{PtCl}_2\text{P}_2\text{Cl}_3$ , and  $\text{PtCl}_2\text{P}_2(\text{OH})_6$ . In such cases, apparently, the higher valencies must be attributed to the *non-metallic* elements. The three compounds,



are well worthy of consideration. It is at least obvious that the supposed dominant valency of the Pt is not concerned, but that the tervalidity of chlorine and the quadrivalidity of oxygen most probably are. And if we remove the so-called but very doubtful "polyad," Pt and examine the following series\*, this will be still more apparent:

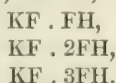


Certainly such a series suggests an analogy to the residual charge of a leyden jar, especially when we bear in mind Maxwell's observation that a dielectric composed of strata of different sorts gives the residual effect, although none of the substances alone do so.

These examples must suffice to show the meaning which may be provisionally attached to the conception of validity, with the following general results:—

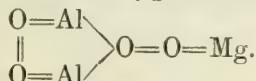
(1) Very high and shifting valencies for the elements are improbable and unnecessary.

\* Another striking set of triplets has just been discovered by Moissan (French Academy of Sciences, Feb. 20, 1888),



Fluorine is indeed clearly tervalid, to say the least; and its very isolation by Moissan seems due to the combination  $\text{K}-\text{F}=\text{FH}$  or  $\text{K}-\text{F}=\text{FII}$ .

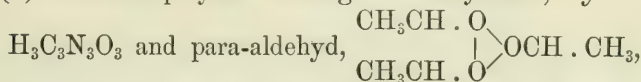
(2) The existence of the so-called molecular compounds is due to the validity or varying valency of the non-metallic radicals, certainly not, as a rule, to the very high validity of the metallic atom. Thus the great difficulty experienced in "washing" certain precipitated oxides free from potash is, I take it, due to the validity of oxygen. And in spinelle,  $\text{Al}_2\text{O}_3 \cdot \text{MgO}$ , the tetravalent oxygen makes it probably



(3) In particular, the numerous grades of bodies containing  $\text{H}_2\text{O}$  is due to the tetravalency (or quadrivalidity) of oxygen conditionally stimulated in some unknown way (say, to a change of vibration-period or to a development of residual charge) by chemical proximity to other non-metallic or negative atom or atoms.

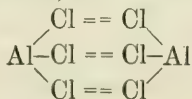
(4) So-called catalytic change is often, if not always, due to this development or existence of validity. This is seen in the behaviour of hydrochloric acid as contrasted with liquid hydrogen chloride.

(5) So-called polymeric changes in many cases, *e. g.*



are similarly often due to the tetravalency of oxygen. These seem to be perfectly analogous to the cases of allotropy.

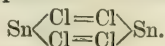
(6) In such cases we must be prepared to extend the idea of *nucleus* hitherto confined to carbon (and extended on unsatisfactory evidence to several of the metals\*) to a chlorine nucleus, an oxygen nucleus, &c. The formula



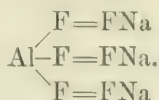
is at least as probable as



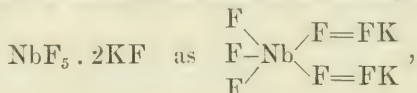
\* Of the numerous cases usually instanced that of tin seems to me alone good. The evidence of  $\text{Sn}_2\text{Et}_6$  is strongly in favour of  $\begin{array}{c} \text{Sn Et}_3 \\ | \\ \text{Sn Et}_3 \end{array}$ ; whereas  $\text{Sn}_2\text{Cl}_4$  at low temperatures is ambiguous, and probably



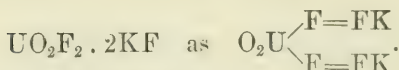
and removes the difficulties which have been raised as to the doubtful trivalent character of aluminium. It certainly turns the scale in favour of the questioned  $\text{Al}(\text{CH}_3)_3$ , and leaves Al trivalent, as its place in the periodic system emphatically requires. No striking a *chemical* substance as the Greenland mineral cryolite,  $\text{Na}_3\text{AlF}_6$ , has hitherto been between the Scylla of the old dot formula,  $\text{AlF}_3 \cdot 3\text{NaF}$ , and the Charybdis of nonad or enneavalent aluminium. But it is difficult to resist the conclusion that fluorine is here trivalent,



In the case of other double fluorides it may be that certain of the atoms are in these circumstances trivalent, *e.g.* in



or



It scarcely seems to follow that two out of the five F atoms in  $\text{NbF}_5$  are not "chemically equal." The questions of "validity," "residual affinity," &c. concern the FK or FNa connexion. These ideas at least seem worth taking as a working hypothesis.

(7) Although it is premature to generalize, it is well worthy of attention that temperature is a function of validity, very often, to say the least, in the direction of change, whether of degree only or not, from validity to valency, *i.e.* to the smaller number. The analogy between polymerism and allotropy then seems to be as close as possible. A discussion of the following parallels would exemplify this idea:—

Hotter.	Colder.
$\text{O}_2$	$\text{O}_3$
$\text{S}_2$	$\text{S}_8$
HF	$\text{H}_2\text{F}_2$
$\text{C}_2\text{N}_2$	$\text{C}_6\text{N}_6$
$\text{C}_2\text{H}_2$	$\text{C}_6\text{H}_6$
$\text{P}_4$	$\text{P}_2$
CO	$\text{CO}_2$
$\text{NO}_2$	$\text{N}_2\text{O}_4$
$\text{K}_2\text{O}$	$\text{K}_2\text{O}_4$
AsI	$\text{AsI}_3$

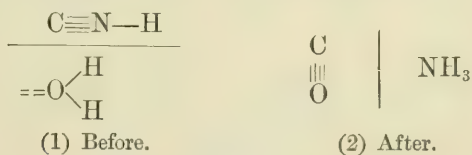


Hotter.	Colder.
ICl	ICl <sub>3</sub>
SnCl <sub>2</sub>	Sn <sub>2</sub> Cl <sub>4</sub>
GaCl <sub>3</sub>	Ga <sub>2</sub> Cl <sub>6</sub>
H <sub>2</sub> O	H <sub>2</sub> O <sub>2</sub>
C <sub>2</sub> H <sub>4</sub> O <sub>2</sub>	C <sub>4</sub> H <sub>8</sub> O <sub>4</sub>
C <sub>2</sub> H <sub>4</sub> O	C <sub>6</sub> H <sub>12</sub> O <sub>3</sub>

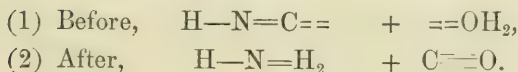
If these speculations are well founded, we ought to be able to predict the discovery of "allotropic forms" of nitrogen and the halogens. The allotropy of six of the non-metals already seems certain; most of the others are open to suspicion: chemists are naturally cautious and conservative on these fundamentals of doctrine. A dirty element may easily be called a new element or another "allotropic form." Nevertheless there are signs of surprises in store for us—dim vistas though they be. In the case of known compounds, also, there may be two metameric prussic acids, viz.



Calling these the alpha and beta "acids," there is some reason for thinking that it is the alpha acid which is formed during Fownes' reaction for CO from K<sub>4</sub>Fe(CN)<sub>6</sub>, and that probably there is hydrolysis of this HCN, thus:—



or

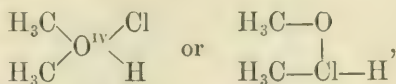


But if so, there ought surely to be some means of distinguishing the nitrile cyanide,  $\begin{array}{c} \text{H} \\ | \\ \text{C}=\text{N} \end{array}$ , from the imide, C≡NH, where

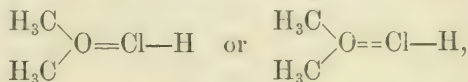
the hydrogen is *not* directly associated with the carbon, though the problem is evidently more delicate than the discrimination of the two bodies formulated C<sub>2</sub>H<sub>5</sub>CNO, or the two striking and odourless C<sub>3</sub>H<sub>5</sub>CNS liquids now known.

The distinction to which it is here endeavoured to draw attention, between the mere raising the valency of *one* *Phil. Mag.* S. 5. Vol. 25. No. 155. April 1888. Y

element and the reciprocally conditional "validity" of negative or non-metallic atoms is well illustrated by the hypothesis of Friedel or Wurtz\*, where Friedel's compound is given as either



as contrasted with the formula now proposed:—



where oxygen can only be tetravalent if chlorine be trivalent, and where, as it may be said, the mutual validity of the oxygen and the chlorine is really one of the points at issue.

Temperature is, then, a function of valency; and Dr. Crookes' address to the British Association served to bring out very clearly that it is also probably a periodic function† of the atomic weight, or at least chemical reacting weight, of an element with *one* alternative valency, as this paper has suggested, rather than many "alternatives" such as the hypotheses of valencies from 2 to 8 or more. The idea is well illustrated by a pendulum arrangement of the periodic series.

Paramagnetic.				Diamagnetic.			
—	<sup>v</sup> N'''	<sup>iv</sup> O''	<sup>iii</sup> F'		Na'	Mg''	Al''' +
+	Se'''	Ca''	K'		<sup>iii</sup> Cl'	<sup>iv</sup> S''	<sup>v</sup> P''' —

The validity alternative valency is indicated on the left of the symbol. It will be interesting to see how future researches on the associations of vortex-rings bear upon this view. At the least it may be urged that it would be very disastrous for chemists to abandon these valence-marks, because the connexions with chemical affinity are not measurable at this stage of the inquiry.

The alternatives of  $(\text{C}_3\text{H}_5)'''$  and  $(\text{C}_3\text{H}_5)'$ , indicated in my first paper, suggest analogies between the probable ordinary monovalent and rarer trivalent behaviour of the halogens. In some cases, also, an element may be trivalent but occasionally univald. But they suggest the further resolution of our present chemical units. If, by way of illustration,  $(\text{C}_3\text{H}_5)'''$  be represented by  $(\text{E})'''$ , we may say we have its "allotropic

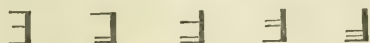
\* See 'Atomic Theory,' p. 250.

† It is curious that the oxygen-group O, S, Se, Te have atomic weights which are *very close* multiples of the oxygen atomic weight.

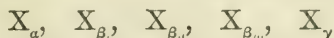
forms "recognizable by the notation



or even



corresponding to the five trivalent groupings



of my previous paper. Thus we have an organic compound radical, long viewed as one of the "elements" of organic chemistry, possessing several clearly distinguished allotropic forms, as it were. On the other hand,  $C_2H_5$  is, so far as our strong evidence goes, a genuine organic "element" without such forms. May there not be some analogous mystery in the case of our present elementary radicals which has yet to be solved, and of which, perhaps, the spectroscope—as foreshadowed by the almost uncanny researches of Dr. Crookes and Prof. Norman Lockyer—already gives us indications? \* Thus Prof. Nordenskiöld's experiments indicate that the crude mixture of yttria, erbia, ytterbia, &c., which Dr. Crookes briefly calls gadolinia, has a constant molecular weight, just as  $X_{\alpha}$ ,  $X_{\beta}$ ,  $X_{\gamma}$  has. Yet the oxide of gadolinium is known to be the oxide of a non-simple body. Meanwhile there is surely enough to cause us to hope for the further great future of what seems to be to some the untenable, if not exploded, theory of valency.

Oxford, 28th January, 1888.

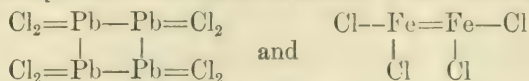
#### POSTSCRIPT.

The importance and difficulties of this subject of validity is seen by Professor Lothar Meyer devoting over fifty pages to it in his 'Modern Theories,' which I have, since writing my two papers, re-read with deep interest after several years' interval. It may be convenient to note briefly our chief differences of opinion.

(1) Oxygen is, as usual, taken as strictly divalent. The result appears to be that chlorine is not only mono- and trivalent, but "probably heptavalent with regard to negative

\* Remembering that oxygen is characterized above all other elements by its extreme chemical *sociability* for others, it is noteworthy that if we add its atomic weight to the atom-weights of the seven elements of the first period, we obtain those of the second period!

elements such as oxygen." On the other hand, the view that in the so-called molecular formula  $KCl$  the elements are only "apparently monovalent" leads the Professor to favour the view of metallic linkings, each metallic atom being associated with strictly monovalent chlorine atoms. Thus formulæ such as



are quoted as probable or possible, pending vapour-density determinations. In face of the scanty evidence for di-metallic linkings in most cases, and in view of the much stronger evidence in favour of trivalent chlorine, I hold at present that the formula  $Fe \begin{smallmatrix} \diagup Cl=Cl \\ \diagdown Cl=Cl \end{smallmatrix} Fe$  is preferable. Moreover the  $K_3Cl_3$  formula seems less probable after the recent vapour-density determination favouring the gas molecule  $K\bar{I}$ . The  $Ag-Ag$  linking is at least premature.

(2) Professor Meyer so thoroughly argues for trivalent aluminium except in  $Al_2Cl_6$ , that only the acceptance of this

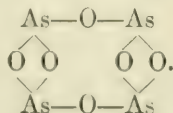
exception to prove the rule as  $Al \begin{smallmatrix} \diagup Cl=Cl \\ \diagdown Cl=Cl \end{smallmatrix} Al$  is required.

It is only in the case of hydrocarbon compounds that the strict monovalence of chlorine can be said to be demonstrated.

(3) Professor Meyer thinks that Ba &c. are divalent in only their more common and most stable compounds, but that "the peroxides indicate a higher valency," presumably  $Ba^{IV}O_2$ . He quotes Geuther as viewing Be, Mg, Ca, Sr, Ba as having valencies 2, 4, 6, 8, and 10. If once the peculiarities of the non-metals in these molecular compounds are recognized, these extraordinary valence powers will disappear. It gives no explanation of the "molecular compounds" which even the "uncommon" compounds with higher valencies themselves form.

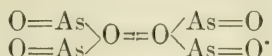
(4) The atoms Ru and Os are viewed as "undoubtedly octovalent" in their volatile oxides  $M^{VIII}O_4$ , and cobalt probably in the purpureo-chloride  $(NH_3)_5CoCl_3$ , as if from  $H_5Co^{VIII}Cl_3$ . The valence numbers are "probably from 1 to 8."

(5) In  $As_4O_6$  Professor Meyer does not link arsenic with arsenic atoms, but suggests, with probability, a symmetrical arrangement:—





Another formula seems also probable, and indicates better what may be called the line of least resistance in the molecule in the quadrivald or tetravalent oxygen



The Professor also states quite clearly his opinion that valency is a whole number; and it is pleasing to quote that, in pointing out that chemists were investigating relations of constitution and atomic linking formerly regarded as subjects which could never be properly investigated, he adds:—"At the present time we consider this research difficult, but we have numerous proofs in the results which we have already gained that it is not only capable of solution, but that it also affords a very fruitful field for the spirit of investigation." It is to hoped that some of the remarkable compounds discussed in these papers will be carefully and minutely examined—J. F. H.

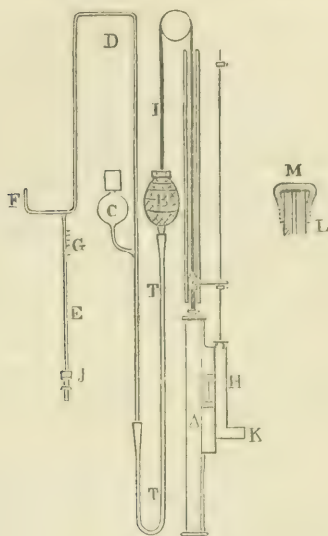
*XL. The Application of Hydraulic Power to Mercurial Pumps.* By FREDERICK J. SMITH, M.A., Millard Lecturer, Trinity College, Oxford\*.

**I**N the mercurial pump of the Töpler type, a cistern of mercury has to be lifted periodically through about four feet. In a form of mercurial pump of this family, devised by the author of the paper, the receiver of the pump has been made very large in comparison with the vessel to be exhausted, in order that the time of exhausting may be diminished as much as possible. The large size of the receiver, holding, as it does, nearly 40 lb. of mercury, increased the utility of the pump greatly, while it added much to the labour of working it. In order to reduce all manual labour to a minimum, the pump has been made self-acting by the addition of an hydraulic motor, having a 4-foot stroke, controlled by a side-valve similar to the D-valve of a steam-engine: this valve is actuated by an auxiliary motor and valve, acted on by a projection from the end of the main piston. The figure shows the disposition of the pump and motor: the motion is quite smooth, and suitable for the work to be done, the motion at the end of the stroke being automatically checked. The cylinder is made of thick-drawn brass tube, the flange being brazed on.

Description of figure:—A is the cylinder, H the valves, K the inlet of the water. The cistern of mercury is attached

\* Communicated by the Author; being abstract of a paper read before the Ashmolean Society, Oxford.

to the head of the piston by means of a leather band, I, which passes over a pulley. A flexible tube, TT, connects this to the globe C. The tube DF leads to the drying tubes and vessel to be exhausted. E is a barometer-gauge dipping into the cistern J, which can be adjusted by means of a screw, so



that the level of the mercury can be brought to the bottom of the scale, G. M L is a full-sized section of the glass-faced valve at the top of the globe C. A projecting arm from the piston embraces a long rod which actuates the valves of the motor.

The framework is not shown, for the sake of clearness.

## XLI. *On some Electrical Phenomena provoked by Radiation.* By Professor A. RIGHI\*.

**I**N seeking for an explanation of the phenomena recently described by Hertz †, by E. Wiedemann and Ebert ‡, and by Hallwachs §, I was led to study the action of light on

\* Preliminary notice communicated by the Author, having been read before the R. Acc. dei Lincei, March 4, 1888.

† Wied. Ann. xxxi. 1887, p. 983.

‡ Ibid. xxxiii. 1888, p. 241.

§ Ibid. xxxiii. 1888, p. 301.

phenomena of contact-electricity between metals. I will give here a brief account of the first results obtained.

(a) A vertical metallic disk A is put more or less close to a metal net B stretched parallel to the disk. Such an arrangement was adopted in order to be able to illuminate those parts of one of the metals which are very near to the other. One of the two metals, A for instance, is in connexion with one of the quadrants of an electrometer of convenient sensibility (a volt corresponding to 130 divisions of the scale); the other, B, is in connexion with the other quadrants and with the earth, while the needle of the instrument is kept at a constant potential (with 100 cells copper-water-zinc). If A is put to earth for an instant, and soon after is conveniently illuminated, a deflection is obtained, which reaches its maximum in a time which is shorter the nearer the irradiating source, and the larger the surfaces of the two metals. The deflection is negative if A is zinc and B brass. The same final value is obtained if A is so initially charged as to have a greater deflection.

If A is very near B, the complete deflection does not change if A is suddenly withdrawn from B, which proves that the radiation has reduced the two metals to the same potential. It follows that this deflection measures the difference of the potential of contact between A and B. If B is placed in connexion with the electrometer instead of A, the deflection is of the contrary sign.

The system of the two metals, when receiving the radiation, behaves then as a voltaic cell, and can be called a *photoelectrical cell*.

The sun's light does not produce the above mentioned effect, at least in a well marked way; the magnesium light is more active; and the light of the voltaic arc gives remarkable results. If the arc be formed between carbon and zinc, the phenomenon shows the greatest intensity, for the electro-metric deflection takes place in a few seconds. This fact leads to the belief that the ultra-violet rays are the most active, which is confirmed by the fact that a plate of glass intercepts the action, while one of quartz diminishes it very little.

(b) Four *photoelectrical cells*, each formed by a disk A and a metal net B, are joined in series and then illuminated. Such a kind of *photoelectrical battery* shows the well-known electrostatic phenomena which are obtained by a battery with open circuit, just as if the metals which form it were plunged in a vessel of water.

(c) If the net in experiment (a) is taken away, and the

radiations are allowed simply to fall on a plate communicating with the electrometer, after the plate has been for an instant put to the earth, a deflection is slowly formed, which is positive with the bodies as yet studied. It seems that in such a case the bodies surrounding the plate replace the metal net; it is therefore probable that negative electricity goes to these bodies and thence to earth.

(d) If A is a brass disk covered with crystalline selenium, it appears that it is at first electronegative to gas-carbon, and that coupling it with a metal net can form a *photo-electrical cell*.

But when the ultra-violet rays are suppressed so as to prevent the occurrence of the phenomenon described, it can then be recognized that the other rays are producing a variation of the electromotive force of contact between the selenium and any other metal, making the selenium more electronegative than when it is in darkness.

Putting aside this last phenomenon, which is of a different kind to those described in *a*, *b*, *c*, and without pretending to attempt to give a complete explanation, which at present would be premature, it seems to me that the idea is at least provisionally acceptable of an electrical convection, provoked by ultra-violet radiations, from bodies on which exists an electrical distribution of a given sign (negative probably), caused by electromotive forces of contact, towards those on which exists an electrical distribution of contrary sign (positive) due to the same cause.

## XLIII. *Proceedings of Learned Societies.*

### GEOLOGICAL SOCIETY.

[Continued from p. 242.]

January 25, 1888.—Prof. J. W. Judd, F.R.S., President,  
in the Chair.

THE following communications were read :—

1. "On *Ailurus anglicus*, a new Carnivore from the Red Crag." By Prof. W. Boyd Dawkins, M.A., F.R.S., F.G.S.

2. "A Contribution to the Geology and Physical Geography of the Cape Colony." By Prof. A. H. Green, M.A., F.R.S., F.G.S.

The account given in this paper of the geology of Cape Colony was founded on observations made during a visit to the country of four months' duration for the purpose of reporting upon the coal. A con-



siderable portion of the colony was traversed by the author, and, owing to the clear atmosphere and the barrenness of the surface, the rocks were unusually well seen. Much, too, had been ascertained by previous observers.

The grouping of the South-African rocks adopted was the following :—

- |                   |   |   |
|-------------------|---|---|
| 9. Stormberg Beds | { | Volcanic Beds, 9 <i>d</i> .<br>Cave Sandstone, 9 <i>c</i> .<br>Red Beds, 9 <i>b</i> .<br>Molteno Beds, 9 <i>a</i> . |
|-------------------|---|---|

8. Karoo Beds.

7. Kimberley Shales.

Great Unconformity.

6. Ecce Beds.

5. Dwyka Conglomerate.

Unconformity.

4. Quartzite of the Zuurbergen, Zwartebergen, and Wittebergen.

3. Bokkeveldt Beds.

2. Table-Mountain Sandstone.

Great Unconformity.

1. Slates and intrusive Granite of the neighbourhood of Cape Town (Malmesbury Beds).

Of the four lowest subdivisions very little was seen. The Bokkeveldt Beds had yielded fossils referred to Devonian. The detailed descriptions commenced with the Dwyka Conglomerate, which was coarse, containing both rolled and angular fragments, the matrix, which was ill bedded, resembling granitic detritus. Some of the boulders suggested doubtfully the action of ice. The Ecce Beds consisted of hardened sandy clays, without lamination, and often weathering in spheroids, and resembling decomposed basalt or dolerite. These beds in the Ecce pass, north-east of Grahamstown, were nearly 5000 feet thick.

The Kimberley Shales were mainly grey and dark sandy shales, with a few thin layers of argillaceous limestone. At their base a conglomerate, resembling the Dwyka Conglomerate, was sometimes found. The Karoo Beds were red and purple shales, with buff or reddish sandstone containing much decomposed felspar.

The Molteno Beds, also sandstones and shales, usually grey- and dark-coloured, associated with grits and conglomerate, contained the only useful coals of the colony. These coals were peculiarly laminated and contained much ash; the seams were destitute of sandstones and often eroded on the upper surface. These characters might indicate subaqueous origin. Owing to the irregularity of the seams, the views generally formed of the coal-resources of the colony may be exaggerated. The upper subdivisions of the Storm-

berg Beds, the Red Beds, shales and sandstones of a red colour, the Cave Sandstone, a massive, fine-grained bed 150 feet thick, weathering white, and the bedded amygdaloidal lava-flows and tuffs that cap the whole, were but briefly noticed, as but few opportunities had offered for examining them.

Some petrological details were given of the contemporaneous and intrusive traps, all appearing to contain the same constituents as the overlying subaerial traps, and doubtless belonging to the same series of volcanic outbursts.

The author proceeded to review the lie of the rocks and physical structure of the country, distinguishing between the area of older rocks near the coast and the later deposits commencing with the Dwyka Conglomerate of the interior. There was apparently unconformity at the base of this conglomerate; it and the overlying Ecca Beds were thrown into folds and occupied the Karoo plains, whilst the ranges to the northward were formed of the higher beds, all nearly horizontal and resting quite unconformably on the Ecca Beds. These ranges had been carved out by denudation, which had removed the Molteno, Karoo, and Stormberg Beds to the south and north. The view advocated by Mr. Dunn that the Kimberley Beds north of the ranges represented the Ecca Beds to the south was discussed, and the author gave reasons for dissenting from it, and classing the Kimberley Beds as a higher subdivision.

Some notes on more recent formations, the conglomerates of Oliphants river and superficial deposits, were followed by a summary of the author's conclusions as to the probable geological history of South Africa. The Bokkeveldt Beds are shown by their fossils to be marine, and possibly all the formations up to the Zuurberg Quartzite may be also marine. The Ecca Beds have yielded no fossils which would enable us to decide whether they are marine or fresh-water; the Kimberley, Karoo, and Stormberg Beds are looked upon as lacustrine.

“On Two New Lepidotoid Ganoids from the early Mesozoic Deposits of Orange Free State, South Africa.” By A. Smith Woodward, Esq., F.G.S.

February 8.—Prof. J. W. Judd, F.R.S., President, in the Chair.

The following communications were read:—

1. “On some Remains of *Squatina Cranci*, sp. nov., and the Mandible of *Belonostomus cinctus*, from the Chalk of Sussex, preserved in the Collection of Henry Willett, Esq., F.G.S., Brighton Museum.” By A. Smith Woodward, Esq., F.G.S.

2. “On the History and Characters of the Genus *Septastræa*, D’Orbigny (1849), and the Identity of its Type Species with that of *Glyphastræa*, Duncan (1887).” By George Jennings Hinde, Ph.D., F.G.S.

3. "On the Examination of Insoluble Residues obtained from the Carboniferous Limestone at Clifton." By E. Wethered, Esq., F.G.S.

The author noticed previous classifications of the Carboniferous Limestone at Clifton, and submitted the following for reference in the present paper :—

*Carboniferous Limestone Series.*

	feet.
Stage C. Upper Limestones.....	100
" B. Middle Limestones .....	1620
" A. Lower Limestones :—	
1. Black Rock .....	490
2. Lower-Limestone Shales .....	500
	990
Total.....	2710

The limestone-forming organisms in each of the above were mentioned, and the methods adopted for obtaining the insoluble residues by means of hydrochloric acid were described. A table of percentages of insoluble residues was given from the Lower Limestone Shales and Black Rock, from the Oolitic Beds, *Mitchelliania*-beds, and main portion of the Middle Limestones, and from the Upper Limestones.

Detrital quartz of small size, with a few grains of felspar, tourmaline, and zircon, characterize the Lower-Limestone Shales, and in one variety the soft tissues of organisms are represented by ferric oxide, which in the case of crinoids represents the whole skeleton. Residues of the Black Rock exhibit slight secondary crystallization round detrital quartz, whilst amorphous and chalcedonic silica become more plentiful. Residues of the Middle Limestone consist to a less extent of detrital quartz along with micro-crystals of quartz, amorphous and chalcedonic silica, and less frequently of pyrites, tourmaline, and zircon; sponge-spicules are also noted. Towards the top of the Middle Limestones the proportion of detrital quartz increases and the deposit of secondary silica on the surface of quartz grains is less marked.

The nature of the amorphous and chalcedonic silica in the limestone, and the relations of this silica to the small quartz crystals, were also discussed. The latter were shown in some instances to possess nuclei of detrital quartz, and where this is not the case, to have resulted from the crystallization of amorphous silica.

February 29.—W. T. Blanford, LL.D., F.R.S., President,  
in the Chair.

The following communications were read :—

1. "An Estimate of Post-Glacial Time." By T. Mellard Reade, Esq., C.E., F.G.S.

The author showed that there exists on the coasts of Lancashire

and Cheshire an important series of Post-Glacial deposits which he has studied for many years. The whole country to which his notes refer was formerly covered with a mantle of low-level marine Boulder-clay and sands, and the valleys of the Dee, Mersey, and Ribble were at one time filled with these glacial deposits.

These glacial beds have been much denuded, especially in the valleys, where the rivers have cleared them out, in some cases, to the bed rock. Most of this denudation occurred during a period of elevation succeeding the deposition of the Low-level Boulder-clay. On this eroded surface and in the eroded channels lie a series of Post-glacial beds of a most interesting and extensive nature. They consist of estuarine silt and *Scrobicularia*-clay covered by extensive peat-deposits, containing the stools of trees rooted into them. Upon these lie, in some places, recent tidal silts, and on the coast margin blown sand and sand dunes. The series of events represented by the denudation of the Low-level Boulder-clay and the laying down of these deposits is as follows:—1st, elevation succeeding the glacial period, during which time the Boulder-clay was deeply denuded in the valleys. 2nd, subsidence to about the 25-feet contour, when the estuarine silts and clays were laid down. 3rd, re-elevation, representing most probably a continental connexion with the British Isles, during which time the climate was milder than at present, and big trees flourished where now they will not grow. 4th, subsidence to the present level, the submersion of the peat and forest-beds, the laying down of tidal silt upon them, and the accumulation of blown sand along the sea-margin extending to a considerable distance in an inland direction.

It was estimated, from a variety of considerations, that these events, all posterior to the Glacial period, represent a lapse of time of not less than 57,500 years allotted as follows:—40,000 years for the elevation succeeding the Glacial period measured by the denudation of the Boulder-clay in the valleys, 15,000 years for the accumulation of the estuarine silts, clays, peat, and forest beds, and 2,500 years for the blown sand.

## 2. "Note on the Movement of Scree-Material." By Charles Davison, Esq., M.A.

The author noticed the frequent high angle of slope of screes, and called attention to Canon Moseley's observations on the downward creep of lead on the roof of Bristol Cathedral, and his subsequent experiment, and stated his belief that stones free to move on the surface of a scree must be affected in the same manner. This he proved by experiments, the result of which he described.

These experiments showed that stones do move downwards, owing to alternate contraction and expansion, the movements accompanying or occurring a short time after the change of temperature, that



the descent is greatest on days of bright sunshine interrupted frequently by passing clouds, and that rain slightly increases the rate of descent.

A description was given of a scree on Hindsearth, Cumberland, in which the stones lie with their longer axes pointing down the slope; and it was pointed out that the movement of the stones in the way described would cause the surface-stones to fall off those on which they rested, and that others would be dislodged during their descent. A numerical estimate was then made of the total amount of movement produced on a scree of a certain size by expansion and contraction of the surface-stones, and after alluding to the relative efficiency of this and other agents upon various screees, the author concluded by pointing out that in the case of the moon this might be almost the only agent at work.

3. "On some Additional Occurrences of Tachylyte." By Grenville A. J. Cole, Esq., F.G.S.

An intrusive sheet, some 8 feet thick, among the basalts of Ardtun Head in Mull, has selvages of tachylyte. The specific gravity of the glass is 2.83, and in other respects it resembles the examples already described from the west of Scotland. But in thin section, numerous fairly translucent spherulites are seen, which accumulate towards the inner part of the selvage until the glassy material between them disappears, and they become polygonal by contact. The rays of these spherulites are often alternately grouped in brown or greyer bundles, both series exhibiting striking pleochroism; but the brown fibres appear darkest when their longer axes are parallel to the shorter diagonal of the nicol's prism, while the greyish and less fibrous areas are darkest in the reverse position. The author believes that two distinct minerals are present, as in the spherulites of the ordinary granophyric structure; the browner rays may be pyroxenic, but the crystalline substances produced under these conditions of hurried consolidation may be far different from those developed in the more central portions of the mass. Spherulites with pleochroic rays are the normal type in basic glasses, and some occur even in some acid examples.

An analysis of the Ardtun spherulitic tachylyte shows it to resemble that of Beal in Skye, having 53 per cent. of silica and nearly 6 per cent. of alkalis.

An occurrence of tachylyte at Kilmelfort, Argyll, was noted, and a description given of an example of great beauty from the Quiraing in Skye. The latter rock shows, in section, a light-brown translucent glass, with abundant cumulates and small brown spherulites with radial structures.

Near Bryansford, County Down, in Ireland, a basalt dyke occurs, the selvage of which must have originally resembled the tachylyte of the Quiraing. The alteration that this glass has undergone guides one in the search for tachylytes (palagonite and so forth) among the Palæozoic rocks of the British Isles, and an instance was

described from Snead, near Bishop's Castle, where fragments of basic glass are imbedded in a tuff of Ordovician age.

In conclusion, the well-known variolite of the Durance was cited as a rock of basic character, comparable, in its perlitic and spherulitic structures with the acid "pyromerides," both types having alike suffered from secondary devitrification.

4. "Appendix to Mr. A. T. Metcalfe's paper 'On Further Discoveries of Vertebrate Remains in the Triassic Strata of the South Coast of Devonshire, between Budleigh Salterton and Sidmouth.'" By H. J. Carter, Esq., F.R.S.

### *XLIII. Intelligence and Miscellaneous Articles.*

#### ON THE PRODUCTION OF INTENSE MAGNETIC FIELDS.

BY PROF. STEFAN.

THE maximum magnetizing force which cylindrical iron cores of an electromagnet can exert between plane pole-pieces standing opposite each other is  $4\pi\mu$ , in which  $\mu$  is the greatest magnetic moment which the unit volume of iron can assume. If we take  $\mu=1700$  absolute units,  $4\pi\mu=213600$ . This value is the more nearly attained, the smaller the distance of the pole-pieces.

The action which the iron cores exert in the centre of the field can be increased without limitation of its length, if the terminal faces of the pole-pieces are made of another shape. This increase attains the greatest value if the ends of the cores have the shape of truncated cones, so that the generating lines of the two conical surfaces pass through the centre of the field, and form with its axis an angle whose tangent is  $\sqrt{2}$ , that is an angle of  $54^\circ 44'$ .

The magnitude of the force is then expressed by the formula

$$H = 4\pi\mu (0.289 + 0.886 \log \frac{d}{a}),$$

where  $d$  is the diameter of the iron core,  $a$  the length of the field. The formula gives an increase of  $H$  to any large values. In practice this is unimportant, for with the slow increase of the logarithms compared with that of the numbers, conditions of construction result which cannot be carried out. If  $H = 8\pi\mu$ ,  $d$  must be taken  $= 85a$ .

For certain optical investigations it is necessary to perforate the iron cores. For such cores with plane pole-pieces the principle no longer holds that the magnetic field is the more intense the nearer the pole-surfaces. The magnetic force in the centre of the field

attains its greatest value if the distance of the pole-surfaces  $a$  satisfies equation

$$a^2 = d_0^2 \cdot \frac{n^{\frac{2}{3}}}{n^{\frac{2}{3}} + 1}$$

in which  $d$  is the diameter of the perforation,  $n$  the number which indicates by how much the diameter of the iron core is greater than that of the perforation.

The perforation of the iron core always produces a great deformation of the magnetic field. The intensity decreases considerably from the centre towards the end of the field. Between plane pole-pieces the force at the end of the field is smaller than the half of the force in the centre. Iron cores with conical ends of the form just prescribed have the advantage of a greater intensity in the centre, and at the same time a smaller decrease of the force towards the end of the field.—*Wiener Berichte*, Feb. 9, 1888.

#### EXPERIMENTS ON THAT FORM OF THE DISCHARGE OF ELECTRICITY KNOWN AS ST. ELMO'S FIRE.

BY A. V. OBERMAYER.

The experiments described in the paper show that the positive brushes are reddish white in the stem, with violet radiating branches. The stems which project into the brush are at angles which may reach  $90^\circ$ , and the rays may attain a length of 6 to 7 centim. Further, that the negative brushes which verge into violet are always so fine that individual rays can no longer be distinguished, that these brushes standing on a luminous point show an angle of aperture of less than  $90^\circ$ , and that the length of the brush is always under 1 centim.

In discharges from positive surfaces, for instance from clothes, these appear covered with fine rays of a length up to 3 centim., which are like the hairs of a fur.

Discharges from similar negative surfaces consist of a phosphorescent glow-light which is in continual motion, and is traversed by dark spaces.

The falls in potential necessary for the production of such a brush-discharge decreases with the distance through which the discharge takes place. In volts per centimetre this fall was approximately :

At 5 centim. distance 6000 volts per centim.			
15	"	4000	"
30	"	1600	"

For distance of a metre this would give about 160,000 volts total difference of potential.—*Wiener Berichte*, Feb. 9, 1888.

INFLUENCE OF LIGHT ON THE THERMAL CONDUCTIVITY OF CRYSTALLIZED SELENIUM. BY MM. BELLATI AND E. LUSANA.

The authors investigated whether light has any effect on the conductivity for heat of crystallized selenium, and have found that such an action does exist. Circular laminae of crystallized selenium were prepared of about 0.03 to 0.04 cm. in thickness, and about 2.5 cm. diameter, and were coated over with a thin layer of the double salt  $\text{CaI}_2, \text{HgI}_2$ . At ordinary temperatures this double salt has a bright red colour, but when heated to  $70^\circ$  it becomes of a dark chocolate-brown. One point of the selenium-plate was then heated by pressing against it the apex of a V-shaped platinum-wire, which was traversed by an electrical current. When thermal equilibrium was established, the chocolate-brown colour had extended to a circle of a definite diameter. The experiment was frequently repeated, partly when the selenium-plate was kept dark and partly when it was under the influence of light. The source was sunlight, which had passed through ammoniacal sulphate of copper in order to eliminate the heat-rays. In each case the diameter of that circle was determined to which the change of colour had extended. The following table may serve as an example, in which D denotes the diameter measured according to a definite unit:—

Without light.		Blue light.	
			126
D.	116		126
	116		126
	115		126
	<hr/>		<hr/>
Mean ..	115.7	Mean ..	126

Hence we have for the ratio of the thermal conductivities, with and without illumination of the areas, 1.13. Light accordingly very appreciably favours conductivity in selenium. The authors show, in conclusion, that the change of electrical as well as of thermal conductivity in consequence of the action of light is of the same order of magnitude.—*Beiblätter der Physik*, vol. xi. p. 818 (from *Atti del R. Ist. Ven.* vol. v. 1887).



THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

MAY 1888.

XLIV. *On the Numerical Relation between the Index of Refraction and the Wave-length within a Refractive Medium, and on the Limit of Refraction.* By T. PELHAM DALE, M.A.\*

THE following equation (see Sir Geo. Airey's Treatise on the Undulatory Theory, ed. 1877, p. 93) is proposed as expressing the relation between wave-length and velocity of propagation in an isotropic medium :—

$$v = \sqrt{\left(1 - \frac{1}{2^{\frac{3}{2}}}\right)^m} \cdot \frac{\sin \frac{\pi h}{\lambda}}{\frac{\pi h}{\lambda}} \dots \dots \dots (\alpha)$$

$v$  is here the velocity of propagation,  $h$  the distance between the undulating particles,  $\lambda$  the wave-length,  $m$  the absolute force of attraction, supposed to follow the law of the inverse square.

For our purpose this may be put under the form

$$v = k \sin \frac{\pi h}{l} \bigg/ \frac{\pi h}{l} \dots \dots \dots (\beta)$$

It is convenient to reserve  $\lambda$  to signify the wave-length in free æther, and to use  $l$  for the wave-length within the medium. The values of the wave-lengths in free æther are those given in the text-book of Glazebrook and the tables of Lupton.

Now, knowing the index of refraction for any fixed line by observation of the index  $\mu$ , the corresponding value of  $l$  is given by the equation

$$l = \frac{\lambda}{\mu}$$

\* Communicated by the Physical Society : read February 11, 1888.  
*Phil. Mag.* S. 5. Vol. 25. No. 156. May 1888. Z



Now  $\mu$  and  $\mu_1$  can be found either by observation or interpolation. If H be the line chosen, then, practically, the index of a wave-length twice as long in the medium will not be far from A ; this we may call  $\mu_{2H}$  and speak of it as the index of the octave below.

It is evident that if  $k$  remain constant,  $\frac{k \sin \theta}{\theta}$  continually approaches  $k$  as  $\theta$  diminishes. Now if  $l$  be increased and  $h$  and  $m$  remain constant,  $\theta$  diminishes ; hence there must be a limit of refraction in the case of waves of very great length. This limit, denoted by  $\nu$ , is evidently found by the equation

$$\nu = \frac{\mu \sin \theta}{\theta} \dots \dots \dots (\epsilon)$$

This is in effect the limit found by Cauchy in a somewhat modified form.

Since

$$\begin{aligned} \nu &= \mu \frac{\sin \theta}{\theta}, \\ &= \mu_1 \frac{\sin \theta'}{\theta'}, \end{aligned}$$

it follows on substitution of  $\frac{\pi h}{l}$  for  $\theta$  and  $\frac{\lambda}{\mu}$  for  $l$ , and dividing out, that

$$\sin \theta = \frac{\lambda}{\lambda_1} \sin \theta_1 \dots \dots \dots (\zeta)$$

Combining this with the equation above,

$$\theta = \frac{l_1}{l} \theta_1, \dots \dots \dots (\eta)$$

we can find a value of  $\theta$  which satisfies both these equations from any two values of wave-length and corresponding index. And from this value of  $\theta$  the corresponding values of  $\theta$  to any other index and wave-length can be found.

In practice this equation can be solved by trial without much trouble, especially when a large number of indices of substances of similar refractive power have to be investigated, when a table may be made once for all, and from this  $\theta$  may be found by inspection.

The wave-lengths made use of in this paper are generally those corresponding to lines A and H. These are both within the visible spectrum, yet far enough apart to render errors of observation of little consequence. In the case of the liquids examined I am altogether indebted to Dr. Gladstone, who furnished me with those contained in Table III. for the purposes

of this investigation. When the line B is given, the octave ray to H may be obtained from A by interpolation, and these values were calculated by this method. It gives results sufficiently near the truth, but has been verified in case of  $\text{CS}_2$  (temp.  $1^{\circ}\text{C}.$ ) and mint hydrocarbon by reference to the two indices A and H, so that the table depends on these two indices only.

It will be seen that in the cases examined  $\frac{\nu-1}{d}$  is constant, within the limits of observation, unaffected by temperature.

Dr. Gladstone and myself, in our joint paper, read before the Royal Society, May 6, 1863, and printed, p. 317, in the Transactions, have pointed out that in consequence of an accidental relation amongst the coefficients used in Cauchy's series, that the equation

$$\nu = \mu_H - 3(\mu_F - \mu_B)$$

holds with considerable accuracy in the cases examined. It appears generally to give a fair approximation to the limit\*, and thus to interpolate indices within the visible spectrum.

While, however, this investigation was in progress Prof. Langley's first paper on the determination of wave-lengths in the invisible spectrum appeared (Phil. Mag. March 1884). He there gives the wave-lengths of lower heat-rays down to length 2.030, and shows that the observed indices of refraction are not only below the limit obtained by Cauchy's formula, but even below those found by other formulæ. The material of his prism was flint glass. I found that the values derived from  $\theta_H$  and  $\theta_{2H}$ , while fairly accurate in the visible spectrum, were increasingly discordant in the longer wave-lengths. If  $\theta_H$  were increased the lower indices were found more accordant; but then the visible spectrum was not adequately represented within the limits of any error which could be imagined possible. This seemed to throw a doubt on the existence of a limiting value. It appeared, however, that the glass prism absorbed the longer waves considerably. The Phil. Mag. for May 1886 contained another paper by Prof. Langley, the material of the prism being rock-salt. On comparing the values found by observation with those furnished by the above equations, the agreement was sufficiently close to encourage investigation, and, accordingly, the calculations were carried further, the approximation being to single seconds in the value of  $\theta$  and every value found.

The results are embodied in Tables I. and II. It will be

\* The divergence in case of  $\text{CS}_2$  is, however, considerable, and  $\nu$  is too large in comparison with that given by the above equations. Especially does this occur in highly dispersive media.



seen in the case of rock-salt that the agreement between the values obtained from the sine—which involve only wave-lengths in free æther—and those obtained from the arc, which involve the two indices, and which should be equal to the former, are very close. The errors, it will be observed, are of small amount and not of uniform sign, possibly, however, showing a small tendency to increase on the side of angle from the sine in the case of the lower indices. Turning to the table of indices in the case of flint glass, we see that the angle for H,  $24^{\circ} 18' 40''$ , gives fairly accordant values down to wave-length 0.940, and not far from the truth to 1.270; after this the values increase in the case of the sine over that of the arc, or, in other words, the calculated index comes out too large. This is the more important because the proportionate increase on so small an angle as  $4^{\circ}$  makes a large increase in the resulting index. If the value of  $\theta_H$  be increased, this difference is diminished in the lower values; but it would require an increase far greater than could possibly be allowed as due to errors of observation in order to include the lower values, and the upper would become greatly discordant with observation.

It was, however, noticed by Prof. Langley that the flint-glass prism appeared strongly absorbent of the lower rays. It would no doubt be unsafe to reason very confidently from results obtained from only two substances. We may remark, however, that the approach to limit of refraction, if it exist, must display itself in observation by a small increase in the index for a large proportionate increase in the wave-length. Any disturbing cause then must become, proportionately, also increasingly effective as the wave-length increases. Hence must arise a tendency to mask the limiting value if attempted to be found by observation. Now in the case of flint glass we can hardly suppose but that some effect is produced by the change of temperature which results from the passage of the heat-rays through the medium. In the case of fluids the heat lowers the index, and thus the limit would be lowered also, and in somewhat greater proportion. It is, in the absence of sufficient data, not easy to say what would be the effect of this change of temperature in solids. In the case of glass, the *course* of the ray through the medium would possibly be raised in temperature above the surrounding mass, and thus there would be a cylinder of glass through which the pencil of rays passed, which might be in a state of constraint as compared with that around it. That a state of constraint exists is shown by the glass “flying” when suddenly heated.

However this may be, it seemed desirable in the first place

to continue the investigation with the data at hand, and to calculate the value of the limit as given by the above equations, using as data two observed indices only. The results are given in the tables, and will be seen to include a considerable number of fluids differing widely in specific gravity, optical properties, and chemical constitution. The list it will be noticed includes certain isomeric bodies and also several differing by a single component.

The first fluid examined was bisulphide of carbon at different temperatures, the data for which were furnished by Dr. Gladstone to me for the purpose of this investigation. It will be seen that the quantity  $\frac{\nu-1}{d}$  is practically a constant.

The same result is observable in the case of mint hydrocarbon and benzene. The range of temperature is not indeed very large, but within the limits taken it is evident that the specific gravity and the quantity  $\nu-1$  increase or diminish *pari passu*, and there is but little doubt that this relationship obtains generally. It would be very desirable indeed to have longer ranges of temperature\* if these could be procured without corresponding errors which high temperature would almost certainly have a tendency to introduce.

It is of interest evidently, in the case of a highly dispersive substance, to determine how far the different indices yield the same limit, as they clearly should do if the formulæ are exact. For the determination of this a value of  $\theta$  was chosen which should yield the value which would satisfy  $\theta_A$ ; and then wavelengths of lines B C D E F and G in free æther and in the medium were treated so as to find  $\theta_B$  &c. If the sine and arc give the same result, then calculation and observation agree. It will be seen that in the case of  $\text{CS}_2$  there is an outstanding difference which amounts to not quite three minutes of arc, and increases from A to H. This is equivalent to saying that the formulæ give correct results to the third place in the case of  $\text{CS}_2$ . This is not so good as the rock-salt in Table II. The index of G is most affected. The difference was noticed by the late Professor Baden Powell, in his 'Undulatory Theory as applied to the Dispersion of Light,' London, 1841. This matter deserves further investigation, but it is a matter also which will involve a considerable amount of calculation; thus it seemed right to postpone this for the present, as being better

\* This is especially desirable near boiling and freezing points. It is possible that the specific gravity at boiling-point might reveal relations at present masked by the arbitrary character of specific gravities taken at any temperature convenient for observation.

policy to obtain first a sufficient number of values of  $\nu$  in case of media varying considerably in optical properties.

It seemed also desirable to include in the choice of bodies several which were isomeric. There is a certain analogy in these to a body at different temperatures, as in either case we are dealing with the same chemical elements. The result is, as will be seen from examining the tables, that

in many cases  $\frac{\nu-1}{d}$  is very nearly a constant. In no case

yet examined is the divergence very great. Thus in benzene and styrolene, cresol, metacresol, and benzil alcohol, benzil chloride and chlorotoluine, methyl citraconate and methyl mesaconate, the agreement is close; but in case of picoline and aniline, as also acetone—with which butyric ether agrees—as compared with allyl alcohol there is a difference of about three units in the second place\*. It may be merely an acci-

dental coincidence, but it appears that the quantity  $\frac{\sqrt{h}}{d}$  in

these two last-mentioned cases is nearly the same, but in others the divergence is found to be greater. This also requires further investigation.

It must, however, be remembered that in all these the limit is derived solely from A and H. It is evident, then, that if either of these indices for A or H are in error, or *are affected by anomalous dispersion*, all the rest will be affected. Yet, considering the nature of the investigation,—which is, given certain indices to find the rest—it is evident that the fewer which are assumed as data the more confidence we may have, if we find that the rest are all calculable within reasonable limits. This, then, is the result of the present investigation: given two indices, all the rest can be found within limits, which are in a considerable number of instances very fair approximations to the truth. As, however, in the cases observed the outstanding differences are in the same direction, and increase apparently towards the more refrangible end of the spectrum, we have an indication of law, which may be sought for both in the circumstances of the experiment and in the mathematics of the problem.

It will be observed that the effect of substitution on the quantity  $\nu-1$  is very marked indeed. This is shown in the case of ethyl sulphide and ethyl thiocarbamide, and still more in that of the substitution of iodine and bromine for chlorine. Iodine is peculiarly effective in increasing the

\* But in these latter the specific gravity is evidently too high in the case of acetone, and in a less degree with butyric ether.

specific gravity in far greater proportion than it raises the value of the refractive and dispersive powers. No simple relation between either density and  $\nu-1$  or the quantity denoted by  $h$  is as yet apparent. I have tried the square and cube, and the square root and cube root of  $h$ , as well as the simple power. I have noticed above that  $\frac{\sqrt{h}}{d}$  is in case

of some isomeric bodies nearly a constant, but this may be a mere coincidence. It is, however, worth noticing, as  $\sqrt{h}$  enters the coefficient  $k$  of Airey's equation cited above. When the logarithms are at hand, the calculations are so simple that even in the absence of any theoretical considerations it seemed worth while to try them.

It will be observed that the equation, by ( $\zeta$ ) and ( $\eta$ ),

$$a \sin \theta = \sin m\theta$$

is impossible if  $a \sin \theta$  is greater than unity. Now as  $a$  is the ratio of two wave-lengths in free æther, denoted in this investigation by  $\lambda : \lambda_1$ , we may ask what happens if the ratio of  $\lambda_1$  the upper wave-length to  $\lambda$  the lower exceeds the ratio unity. Now in all cases yet examined, with the exception of selenium, this shorter wave-length is beyond violet and indeed the visible spectrum. In selenium it must lie somewhere near E; in solid phosphorus and sulphur it is beyond the furthest limit of the spectrum towards its more refrangible end, given by Prof. Langley, *i. e.* 0.3727. The results are given for these three substances in Table IV. annexed. It will be seen that phosphorus reaches the limit at about 2684 and sulphur at nearly 2664. We may observe as a coincidence that the behaviour of selenium in relation to light is remarkable, while its red colour shows that the violet end of the spectrum is absorbed. The results in Table V. are supplemental, calculated to the nearest minute, from determinations recently supplied by Dr. Gladstone, of  $\nu$  for water and alcohol.

#### *Abstract of Results.*

1. That the relationship between the limit of refraction ( $\nu$ ) found by the equation  $a \sin \theta = \sin m\theta$ , where  $a$  is the ratio of two wave-lengths in free æther and  $m$  the corresponding ratio in the medium, give a value which has the property expressed by the equation  $\frac{\nu-1}{d}$  a constant, where  $d$  is the density of the medium and  $\nu$  its limit of refraction at the same temperature.



2. That in isomeric bodies the same relation obtains closely in a large number of the bodies examined.

3. In all the cases examined  $\nu$  is a quantity not *very* far below  $\mu_A$ .

4. That the substitution of one chemical element for another has a very great influence on the quantity  $\frac{\nu-1}{d}$ , and substances with high chemical equivalent have, as far as examined, a far greater influence in increasing the density than in raising the limit.

5. That the equation  $a \sin \theta = \sin m\theta$  gives in the cases examined an approximate value for all the other indices, two being assumed as data. In the case of rock-salt this holds good far down in the invisible heat-spectrum, and also in the ultra-violet, but fails somewhat at a certain point in the flint-glass spectrum, and also, to a less degree, at the upper end of the visible spectrum of bisulphide of carbon.

It is evident that for a certain value of  $a$  and  $\theta$  the equation  $a \sin \theta = \sin m\theta$  becomes impossible. This would mean that there is an upper limit which is given by a certain value of wave-length, beyond which the formula gives impossible results. In the case of selenium only, of the substances examined, is this limit within the visible spectrum. The analogy with the critical angle of total reflexion gives this circumstance considerable interest.

It is evident, then, in the present state of our data, that in all cases examined the formula of Sir Geo. Airey gives an approximate value of an index by means of two others, and in this way they can be calculated; but that, as a rule, the lower indices come out too high as compared with the upper. A cause for this may perhaps be suggested in the heating of the medium due to the absorption of the rays; and as there seems to be evidence of absorption of short waves above the longer ones, given by recent stellar photometric determinations, this absorption would perhaps, if included in the conditions of the problem, account for some of the outstanding difference. This and anomalous dispersion—itsself apparently depending on absorption—opens an interesting field of inquiry, and in this direction I propose to pursue the investigation.

Since the above was written I have been informed by Dr. Gladstone that he in some cases used a screen of solution of alum, but found that practically nothing was gained by its use. This fact, then, must be taken into account in any attempt to explain the outstanding differences between theory and observation.

TABLE I.—LANGLEY'S FLINT-GLASS PRISM.

Line.	Wave-length.	Index.	$\theta$ from sin.	$\theta$ from arc.	$\theta$ from sin.	$\theta$ from arc.
O.....	0.34400	1.6266	28° 21' 6"	28° 22' 50"	29° 16' 30"	29° 17' 10"
H ...	0.39679	1.6070	<b>24 18 40</b>	do.	<b>25 5 0</b>	
F.....	0.48606	1.5899	19 58 10	19 38 7	20 14 50	20 15 33
D.....	0.58890	1.5798	16 6 20	16 6 26		
C.....	0.65618	1.5757	14 24 57	14 24 57	14 51 15	14 52 25
A.....	0.76009	1.5714	12 24 20	12 24 20		
	0.815	1.5697	11 33 46	11 33 42		
	0.850	1.5687	11 4 50	11 4 43		
	0.890	1.5678	10 34 30	10 34 28		
	0.910	1.5674	10 20 30	10 20 23		
	0.940	1.5668	10 0 21	10 0 21		
	1.130	1.5636	8 18 43	8 18 23		
	1.270	1.5616	7 23 19	7 22 53		
	1.360	1.5604	6 53 56	6 53 15		
	1.540	1.5576	6 9 30	6 8 31	6 20 30	6 20 13
	1.580	1.5572	5 56 0	5 54 20		
	1.810	1.5544	5 10 40	5 9 10	5 19 50	5 19 7
	1.870	1.5535	5 0 40	4 59 10		
	1.980	1.5520	4 43 55	4 42 20		
	2.030	1.5515	4 37 11	4 35 17	4 44 32	4 41 1

TABLE II.—ROCK-SALT PRISM.

(Prof. Langley's Determinations, Phil. Mag.  
May 1886, no. 132.)

	I. Wave-length.	Index.	$\theta$ from sine.	$\theta$ from arc.	Difference.	Limit of Refract.	
						Sine.	Arc.
M ...	0.3727	1.57486	24° 46' 35"	24° 46' 45"	+0' 10"		
L.....	0.3820	1.57207	24 8 5	24 8 19	0 14		
H <sub>2</sub> .....	0.3933	1.56920	23 23 50	23 24 8	0 18		
H <sub>1</sub> .....	0.3968	1.56833	<b>23 10 49</b>	datum.		1.52570	
G.....	0.4303	1.56133	21 17 0	21 17 7	0 7		
F.....	0.4861	1.55323	18 44 47	18 44 47	0 0		
b <sub>4</sub> .....	0.5167	1.54991	17 35 41	17 35 40	-0 1		
b <sub>1</sub> .....	0.5183	1.54975	17 32 19	17 32 16	-0 3		
D <sub>1</sub> .....	0.5889	1.54418	15 22 49	15 22 49	0		
D <sub>2</sub> .....	0.5895	1.54414	15 21 50	15 21 52	+0 2		
C.....	0.6562	1.54051	13 46 12	13 46 12	0		
B.....	0.6867	1.53919	12 8 49	12 8 50	+0 1		
A.....	0.7601	1.53670	11 51 12	11 51 13	+0 1		
$\rho$ .....	0.94	1.5328	9 33 53	9 33 58	+0 5		
$\phi$ .....	1.13	1.5305	7 56 42	7 56 40	-0 2		
$\psi$ .....	1.38	1.5287	6 29 49	6 29 51	+0 2		
$\Omega$ .....	1.82	1.5268	4 55 24	4 55 14	-0 10	1.5251	1.5249

$H_1$  is found from indices of  $H_2$  and A, and from H all the other lines are deduced. Molecular distance  $h=0.032570$ ;  $\mu n-3(\mu_F-\mu_n)=1.52621$ .

TABLE III.

Bisulphide of Carbon.							
Temp. <i>t.</i>	Sp. Grav. <i>d.</i>	Index A. $\mu_A$ .	Index H. $\mu_H$ .	Angle A. $\theta_H$ .	Limit. $\nu$ .	Mol. dist. <i>h.</i>	Specific Limit. $\frac{\nu-1}{d}$ .
1.5	1.2909	1.6227	1.7159	18 38' 2"	1.5944	.048505	46048
23.0	1.2594	1.6070	1.6972	18 25 50	1.5799	.04844	46046
38.0	1.2494	1.6026	1.6922	18 25 30	1.5752	.048559	46038
Another Specimen.							
10.0	1.2793	1.6153	1.7078	18 37 43	1.5869	.048673	45885
24.5	1.2593	1.6045	1.6954	18 30 45	1.5770	.048616	45819
Comparison of Values of Limit calculated from sine $\theta_H$ and arc $\theta_H$ with corresponding angles for CS <sub>2</sub> at 10°.— $\theta_H = 37^\circ 44' 40''$ .							
	Line A.	B.	C.	D.	E.	F.	G.
Sine ...	18 37' 43"	20 42' 50"	.....	24 19' 59"	.....	29 54' 4"	34 19' 40"
Limit...	1.5870	.....	.....	1.5862	.....	1.5843	1.5847
Arc ...	18 39 46	20 41 52	.....	24 18 52	.....	29 55 58	34 16 52
Limit...	1.5869	.....	.....	1.5861	.....	1.5849	1.5848
Another value obtained by interpolation, $\theta_H = 37^\circ 29' 20''$ .							
Sine ...	18 35' 20"	20 40' 10"	21 40' 40"	24 16' 50"	27 23' 30"	29 54' 50"	34 13' 40"
Arc ...	18 35 10	20 40 21	21 40 21	24 15 30	27 21 10	29 51 50	34 11 10
The value of $\nu$ lies between 1.5867 and 1.5852.							
Mint Hydrocarbon.							
Temp. <i>t.</i>	Sp. Grav. <i>d.</i>	Index A. $\mu_A$ .	Index H. $\mu_H$ .	Angle A. $\theta_A$ .	Limit. $\nu$ .	Mol. dist. <i>h.</i>	Specific Limit. $\frac{\nu-1}{d}$ .
6.5	0.8926	1.4728	1.4988	11 4' 50"	1.4632	.....	51893
25.0	0.8820	1.4650	1.4901	10 58 0	1.4561	.....	51712
Benzene.							
2.0	0.8979	1.5021	1.5460	13 56 40	1.4873	.039219	54272
23.7	0.8760	1.4893	1.5320	.....	1.4725	.039122	54328
28.6	0.8709	1.4860	1.5279	13 50 35	1.4720	.038892	54196

Table III. (*continued*).

Benzene.—Another Specimen.							
Temp. <i>t.</i>	Sp. Grav. <i>d.</i>	Index A. $\mu_A$	Index II. $\mu_{II}$	Angle A. $\theta_A$	Limit. <i>v.</i>	Mol. dist. <i>h.</i>	Specific Limit. $\frac{v-1}{d}$
10.0	0.8868	1.4935	1.5355	13 43 10"	1.4792	.038802	54037
21.5	0.8773	1.4887	1.5304	13 41 45	1.4745	.038850	54077
Styrolene.							
11.0	0.9409	1.5208	1.5693	14 1 0	1.5057	.038848	53747
Another Specimen.							
							53853
Cresol.							
23.0	1.039	1.5316	1.5787	14 45 0	1.5137	.040776	49442
Metacresol.							
19.0	1.033	1.5257	1.5726	14 13 50	1.5102	.039395	49390
Benzil Alcohol.							
22.0	1.0412	1.5278	1.5710	13 58 0	1.5150	.038089	49467
Methyl Citraconate.							
15.5	1.1164	1.4442	1.4721	11 32 0	1.4345	.....	38920
Methyl Mесаconate.							
16.0	1.1246	1.4492	1.4813	12 19 0	1.4380	... ..	38938
Picoline.							
23.5	0.94093	1.4912	1.5317	13 29 0	1.4774	.038247	50726
Aniline.							
13.0	1.016	1.5695	1.6336	16 9 0	1.5488	.043342	54016
Another specimen.							
7.5	1.0322	1.5780	1.6449	16 25 0	1.5565	.043940	53920
Benzil Chloride.							
7.0	1.099	1.5314	1.5764	13 59 0	1.5160	.037148	46592
Chlorotoluine.							
19.0	1.0761	1.5173	1.5613	13 54 0	1.5027	.038701	46717



Table III. (continued).

Acetone.							
Temp. <i>t.</i>	Sp. Grav. <i>d.</i>	Index A. $\mu_A$	Index H. $\mu_H$	Angle A. $\theta_A$	Limit. <i>v.</i>	Mol. dist. <i>h.</i>	Specific Limit. $\frac{v-1}{d}$
*25.5	0.8117 at 15°	1.3540	1.3706	9 25 0	1.3479	.024733	42860
Butyric Ether.							
*23.0	0.8778 at 20°	1.3850	1.4669	9 24 0	1.3778	.027995	43039
Allyl Alcohol.							
23.0	0.8563	1.4054	1.4289	10 44 55	1.3695	.025438	46304
Allyl Sulphide.							
11.0	0.8544	1.4531	1.4811	11 30 40	1.4434	.....	51837
Ethyl Thiocarbamide.							
18.0	1.0030	1.5040	1.5477	13 13 0	1.4890	.....	48794
Orthobromotoluene.							
.....	1.4192	1.5502	1.5981	14 3 50	1.5299	.....	37340
Compare this with benzil chloride above					1.5160	.....	46592
Isobutyl Chloride.							
19.0	0.8626	1.3939	1.4111	8 57 30	1.3882	.....	45004
Isobutyl Iodide.							
.....	1.5982	1.4874	1.5240	12 53 0	1.4749	.....	29717
Mercuric Methyl.							
30.0	2.966	1.5229	1.5683	14 5 0	1.5076	.. ...	17365
Bromopierin.							
13.0	2.816	1.5736	$\mu_D$ 1.5831	14 0 0	1.5246	.....	18612

\* It will be seen that in both these *d* is too great.

TABLE IV.

Substance.	Lower Index.	Upper Index.	Angle $\theta_A$ .	Limit.	Limiting Wave-length.
Selenium {	$\mu_A$ . 2.653	$\mu_D$ . 2.980	$\theta_A$ . 44° 9' 0"	<i>v.</i> 2.1353	5295.7
Solid Sulphur {	$\mu_A$ . 1.9024	$\mu_E$ . 1.9527	20° 30' 0"	1.8621	2663.8
Solid Phosphorus {	$\mu_A$ . 2.1059	$\mu_E$ . 2.1442	20° 40' 0"	2.0611	2683.8

TABLE V.

Sp. g.	$\mu_{\Delta}$ .	$\mu_{\Pi}$ .	$\nu$ .	$\frac{\nu-1}{d}$ .
Water. 1.000	1.32924	1.34393	1.3249	3249
Alcohol. 0.7919	1.3585	1.3735	1.3531	44589

XLV. *Tidal Action as an Agent of Geological Change.*

By T. MELLARD READE, C.E., F.G.S., F.R.I.B.A.\*

THE nature of tidal action, as regards its effect on the sea-bottom, seems to be insufficiently understood, if not in many cases actually misapprehended.

In looking over most of the late works on Geology this fact has been much impressed upon me. Very little influence seems to be attached to the tides as agents of geological change, and then only as surface-currents affecting the coasts and shores. So long ago as 1873, in a paper entitled "*Tidal Action as a Geological Cause*"†, I attempted to show that the mechanical action of the tides was deep and wide spread, affecting not only the littoral, but the whole bottom of the sea, while the profoundest depths of the ocean are not entirely exempt from tidal movement.

My present object, while recalling attention to this subject, is to adduce some further facts in support of the important office the tides perform as distributors of material worn from the land, and even in special cases as agents of erosion acting on the sea-bottom.

The force producing the tidal-wave affects every particle equally, down to the most profound depths of the ocean, while the forces creating wind-waves act only on the surface. A cork floating upon deep water disturbed only by the ordinary waves, revolves (as do also all the surface-particles of water) either in a vertical circle or an ellipse, not very different from one having the longer axis vertical. In the free tide-wave

\* Communicated by the Author.

† Proceedings of the Geological Society of Liverpool, Session 1873-4.

produced by the action of the sun and moon the particles move in an excessively elongated ellipse, the shorter axis of which is vertical.

Sir John Herschel clearly shows "that all the water that goes to form the elevated portion of the tide-wave must be brought from the depressed area ; and this can only take place by a lateral approach of the vertical sections of the sea when the water is rising, and their recess from each other when falling, which is only another way of expressing an alternating backward and forward movement."\* It is evident, from these considerations, that we may practically consider the particles of water in tidal movement to be differentiated in velocity only by frictional retardation on the sea-bottom, in other words, by work done on the materials constituting it.

It is necessary to explain that, in the free ocean, the total height of the tide-wave between high and low water is but small, and the advance and recess consequently not rapid ; but by the concentration of forces the movement of an immense mass of water, though at first slow, advancing up a gradually constricted channel increases rapidly in velocity as the sectional area diminishes, and a derivative wave is created.

With this bare statement of principle we will proceed to question recorded phenomena, and see if it is borne out by the facts of nature.

*Evidences of Bottom-velocity.*—Between Glas island and Sgeir-i-Noe, in the Little Minch off the west coast of Scotland, the flood-stream often takes the buoys of the long lines down ; "and it is a remarkable circumstance, indicative of the great depth of the tidal-stream here, that the buoys, though anchored in 70 or 80 fathoms, are taken completely to the bottom ; star-fish and other marine animals being found attached to them."† As in fine weather the surface-velocity here is only  $1\frac{1}{2}$  knots per hour, it seems pretty evident the bottom-velocity in this case must exceed that at the surface.

In the Gulf of Coirebhreacain, between Scarba and Jura, the tide runs with a velocity, by actual test, of  $9\frac{1}{2}$  miles an hour. The principal stream of flood enters from the eastward from the Sound of Jura, and runs out in a dangerous race about N.W. for about halfway towards Colonsay, forming eddies from the island of Scarba on its western side.

During the time that the stream of flood runs westward through the middle of the Gulf there are counter-streams

\* *Physical Geography*, p. 63.

† 'Sailing Directions for the West Coast and Islands of Scotland from the Mull of Cantyre to Cape Wrath,' p. 119.

running eastward along both sides of the Gulf, which contribute to increase the agitation of the waters\*.

The width of the channel is about two thirds of a mile, and its greatest depths from 80 to 105 fathoms, while in the Sound of Jura and in the sea to the westward the soundings are generally much less. The greatest depth occurs just in the position it should do if attributable to the excavating power of the current. One of the soundings of 86 fathoms on my chart shows a rock bottom, so probably the tide sweeps the bottom of the channel as deeply as the rock will allow.

To the west of Scarba, and turning at an angle from the Coirebhlreacain channel in the direction of Loch Linnhe, is a depression or "deep," with soundings of from 113 to 124 fathoms. It is very probable that the action of the tide in these complicated channels between the numerous islands produces under-currents, having a direction diverse from and perhaps sometimes opposite to the surface-streams.

To the eastward of Barra Island, from south of Barra Head to Barra Sound, a distance of over 30 miles and parallel to the land, is a trench from 100 to 132 fathoms deep; while outside the general depth is about 80 fathoms with a muddy bottom. In this trench four of the soundings show a rocky bottom at depths of 104, 125, 128, and 132 fathoms; while one of 131 fathoms is marked as stony, the remainder being sand, shells, or mud. Here, again, it is evident that the tidal scour has eroded this channel at a depth of 780 feet.

In the Little Minch there is a "deep" between the Isle of Skye and North Uist with a sounding of 107 fathoms, "rock," the surrounding bottom, with soundings from 50 to 90 fathoms, being shells, sand, or mud.

Between the island of Rum and the island of Barra there are a good many soundings showing rock, which are deeper than others in the vicinity which have muddy bottoms.

Off the island of Rathlin, north of county Antrim, there is a deep or pool with soundings of 100 to 133 fathoms; several of the deepest show rock. The surrounding bottom is of shells, sand, and mud at less depths.

It seems to me pretty evident that, as in the case of the "remarkable ditch," mentioned by Captain Beechy, off the coast of Wigtonshire, 20 miles long by about a mile only in width, and from 400 to 600 feet deeper than the general level of the bottom about it, these "deeps" off the Scotch and Irish coasts have also been "scooped out" by the tide†.

\* 'Sailing Directions, West Coast of Scotland,' p. 43.

† See "Tidal Action as a Geological Cause," Proc. Geol. Soc. of Liverpool, 1873-4.



It is true that some of these depressions have been claimed by Prof. James Geikie as the work of glaciers, but there seems to me insufficient grounds for that assumption. In the first place, before it could be admitted, we should have to prove that these deeps are rock-basins; which, as the surrounding bottoms are all, or nearly all, mud and sand, it would be difficult to do.

They may or may not be rock-basins; but even if we admit that glaciers could, under certain conditions, scoop out depressions in a rocky floor in such positions (a question on which there exists much difference of opinion), these channels or depressions could not have been cleared of the deposits which formerly filled them, or afterwards have been kept open, except by the action of sea-currents.

Thus it is seen that, while the form of the channels and coast-lines affect the direction and velocity of the tidal currents, the tidal flow reacts under favourable conditions and increases the depths of the channels. Thus the actual erosive influence on a rocky bottom may be small; on a bottom of softer materials it may be large, sweeping away the looser sands, gravels, or clays until it reaches the rock, or until its force is expended and an equilibrium of velocity and depth be attained. It is at all events remarkable that in all the deep pools I have mentioned, some of the deepest soundings disclose rock while the shallower ones do not.

This seems to me irreconcilable with the glacial theory of the origin of these "deeps."

It will not follow from the principles I have attempted to enunciate that the deepest channels are always situated where there is the swiftest current. The velocity of the current, on the contrary, is often increased by the sudden shoaling of the water, so that the most dangerous and impetuous tides may be caused by rocky ledges which resist erosive action.

The deeps, I take it, are the result of the steady movement of a great body of water, having usually a bottom-velocity probably not much less than that of the surface, and in some cases more; and the depth of erosion is to a large extent determined by the nature of the materials upon which the moving waters act.

*The Tide as a Distributer.*—The foregoing examples have all been taken from a coast where the contours and islands give great diversity and strength to the currents, and where the erosive action is consequently at its greatest. But, as a distributor of material, the tide performs perhaps a greater function. In less restricted areas it is not so forcible in action, but spreading over a larger area the amount of material dealt

with is infinitely greater. It is by tidal influence that the material brought down by continental rivers and that frayed by waves and currents from the coast is gradually worked out to the ocean depths. The North Atlantic is distinguished by a more widespread distribution of sediment over its floor than most of the oceans, doubtless due, as I have elsewhere pointed out, to the more numerous and greater sedimentary contributions of the rivers draining the surrounding land areas.

Without tidal action this sediment would be left to shoal the inshore waters instead of being gradually worked out to great depths, which is what actually takes place. Unfortunately at present our knowledge of the nature of the ocean-bottom is extremely limited, and quite insufficient to justify the wide generalizations of some too-ardent naturalists who would paralyze inquiry by their rigid systems.

*Tidal Movements in the Ocean.*—Although it is on the littoral margins and the shallow seas opening into the oceans that the resistless force of the tides is the most obvious, the tidal influence is felt in the profoundest depths of the ocean. Sir John Herschel says that “a tide-wave of 4 feet of total height, advancing over a sea 30,000 feet deep, implies an advance and recess of 2800 feet, which being spread over six hours is nowhere very rapid.”\*

The mean depth of the ocean is probably not more than 12,500 feet†, and there is every reason to believe that the slow movements of these enormous bodies of water, by concentration in deep channels favourably circumstanced, get translated into rapid currents that scour the bottom. Formerly it was supposed that the waters of these great depths were profoundly and universally still, but the information gained in laying deep-sea cables proves that swift currents sometimes prevail. Sir James Anderson, the eminent telegraph-engineer, says, in a letter to me:—“Perhaps the most marked experience we have had of currents at great depths was in the case of the Falmouth cable near Gibraltar. At 500 fathoms the wire was ground like the edge of a razor, and we had to abandon it and lay a cable well inshore. Captain Nares, of the surveying ship ‘Nemesis,’ I think with tangles could get no specimen of the bottom whatever, and he thinks he got sufficient evidence to prove the existence of a perfect swirl at that depth.”‡

\* 30,000 feet is deeper than the deepest soundings yet recorded.

† Dr. John Murray makes it 2076 fathoms (*Scottish Geographical Magazine*: paper read before the Royal Society of Edinburgh, December 19, 1887).

‡ Quoted by me in “The North Atlantic as a Geological Basin.” Presidential Address, Liverpool Geol. Soc. Session 1885-6.

Mr. Edward Stallibrass, in a valuable paper on Deep-sea Sounding in connexion with submarine Telegraphy\*, says:—"Of all bottoms these ooze are the ones to be preferred. The fact of their being found shows that no currents exist in those parts, and they are so soft that the cable sinks far down into them. The old idea that currents do not exist at any great depths has long since been rejected. Currents may exist at almost any depth. Between the Canary Isles there are strong currents 1000 fathoms below the surface, and their scouring action may be clearly detected."

Mr. J. Y. Buchanan, in the discussion on Mr. Stallibrass' paper, also said:—"If we find hard ground we know that there must be something to prevent the accumulation of sediment. Now the only thing that prevents the accumulation of sediment is a current; and one help that telegraph soundings have thus given to geographical science is the indication that tidal currents exist even at very great depths in the open ocean."†

I have very little doubt that these currents are of tidal origin. Mere differences of specific gravity in the ocean water, whether due to temperature or salinity, are insufficient, in my opinion, to produce such rapid currents; but the arrest and concentration of the immense mass of ocean water set in movement by the attraction of the sun and moon are more potent influences. The comparative neglect of tidal action as a geological cause is doubtless due to our usually looking at the tides as superficial currents; whereas, unlike currents of the Gulf-stream type, they extend to the bottom of the deepest oceans. The subject is one capable of much development; and numerous other illustrations could doubtless be brought forward to fill up this merely outline-sketch which would sustain the main principle I have sought to establish, namely, that tidal action as a physical agent is not confined to coasts and shallow seas and estuaries, but is a widespread and potent cause of geological change.

---

XLVI. *Mathematical Spectral Analysis of Magnesium and Carbon.* By Dr. A. GRÜNWALD. (*Abstract.*)

[For this Abstract the Editors are greatly indebted to Prof. Liveing.]

THE author's method of analysis consists, in general, in tracing a simple numerical relation between the wavelengths of some of the lines in the spectrum of one substance A, and those of lines in the spectrum of some other substance B.

\* Journ. of the Society of Telegraph Engineers, p. 509 (1887).

† Ibid. p. 514.

The group of lines thus harmonically related the author infers to be due to a common component of A and B, and the ratio of the wave-lengths to be dependent on the influence of the other components. He had previously applied his method to the analysis of the spectra of hydrogen and oxygen, and concluded that hydrogen is composed of two primary elements,  $a$  and  $b$ , and has the chemical formula  $ba_4$ ; and that  $a$  is the substance which, when uncombined, produces the "coronal line" and some others in the solar atmosphere (hence called "coronium"), while  $b$  is the substance which, when uncombined, gives the lines ascribed to "helium" ( $D_3$  &c.). Oxygen he thinks to be composed of a substance  $H'$  combined with another substance  $O'$ ; this substance  $O'$  being a compound with the formula  $b_4O_5''$ , where  $O''$  is again a compound  $b_4c_5$ , so that  $O = H'b_4(b_4c_5)_5$ . How he arrived at these particular formulæ he reserves for publication in a future more complete memoir. The spectrum of  $O''$  will include a group of lines due to  $b$  which will be harmonically related to a corresponding group in the spectrum of hydrogen; but inasmuch as  $b$  is combined in the one case with  $c$  and in the other case with  $a$ , and the proportions in which it is combined are also different in the two cases, the wave-lengths of the group of lines are not identical in the two spectra, but are only harmonically related. Again, the wave-lengths of lines due to  $b$  in the spectrum of  $O''$  when it is free will be, not identical with, but harmonically related to the corresponding lines in the spectrum of oxygen in which  $O''$  occurs under the constraint of combination with  $b$  and  $H'$ .

The criteria by which the primary elements  $a$ ,  $b$ ,  $c$  may be recognized are as follows:—

I. If  $\lambda$  be the wave-length of the group of rays produced by  $a$  as it exists in hydrogen:

(1)  $19/30\lambda$  will be the wave-length of the corresponding ray in the spectrum of water, due to  $a$  in the state in which it exists in hydrogen in the state in which the latter occurs in water-vapour; and

(2)  $3/4\lambda$  & (3)  $56/75\lambda$  will be the wave-lengths of two other corresponding rays in the water-spectrum.

II. If  $\lambda$  be the wave-length of one of the group of rays due to  $b$  in the state in which it occurs in hydrogen under the influence of  $a$ :

(1)  $4/5\lambda$  will be the wave-length of the corresponding ray in the water-spectrum due to  $b$  as it exists in hydrogen in the state in which the latter occurs in water;

(2)  $46/41\lambda$  will be the wave-length of the corresponding ray of the oxygen-spectrum due to  $b$  as it exists in  $O'$  in the state in which the latter occurs in free oxygen; and



(2a)  $23/32 \times 46/41 \lambda$  will be that of the corresponding ray of the water-spectrum due to  $b$  as it exists in  $O'$  in the state in which the latter occurs in the combined oxygen of water ;

(3)  $70/59 \lambda$  will be a wave-length of a ray of the oxygen-spectrum due to  $b$  as it exists in  $O''$  in the state in which the latter occurs in  $O'$  in free oxygen ; and

(3a)  $21/32 \times 70/59 \lambda$  will be the wave-length of the corresponding ray of the water-spectrum due to  $b$  as it exists in  $O''$  as the latter exists in  $O'$  in the combined oxygen of water.

III. If  $\lambda$  be the wave-length of a line in the oxygen-spectrum due to  $c$  as it exists in  $O''$  as the latter exists in  $O'$  in free oxygen :

(1)  $3/5 \lambda$  will be the wave-length of the corresponding ray of the water-spectrum due to  $c$  as it exists in  $O''$  in  $O'$  in the combined oxygen of water (chief criterion) ;

(2)  $5/8 \lambda$  will be the wave-length of another line of the group in the same spectrum due to  $O''$ , which group includes other rays besides those referred to in the last preceding paragraph.

IV. If  $\lambda$  be a wave-length of free helium, that is of helium in the state in which it occurs along with coronium uncombined, when hydrogen is dissociated at high temperature and diminished pressure, and its volume thereby increased in the ratio  $3/2$ :

$2/3 \lambda$  will be the wave-length of the corresponding ray of the hydrogen-spectrum due to  $b$  as it is chemically combined in hydrogen ; and  $\lambda' = 2/3 \lambda$  will satisfy the criteria above given under II., so that  $4/5 \lambda'$ ,  $23/32 \times 46/41 \lambda'$ , and  $21/32 \times 70/59 \lambda'$  will be the wave-lengths of three lines of the water-spectrum, and  $46/41 \lambda'$  and  $70/59 \lambda'$  those of two lines of the oxygen-spectrum.

V. On the other hand, if  $\lambda$  be the wave-length of a ray of free "coronium,"  $2/3 \lambda (= \lambda')$  will be the corresponding wave-length in the group of lines of hydrogen due to the combined "coronium," and will satisfy the criteria above given under I.; so that  $19/30 \lambda'$ ,  $3/4 \lambda'$ , and  $56/75 \lambda'$  will be very nearly the wave-lengths of three lines of the water-spectrum.

VI. The wave-lengths of all the lines of the elementary line-spectra of hydrogen and oxygen, when multiplied by  $2/3$  (the factor of condensation of the gases in the formation of water-vapour), become wave-lengths of corresponding lines in the spectrum of water.

Applying his analysis to magnesium, the author finds that the lines of the magnesium-spectrum fall into four groups. Of these the first consists of the lines for which  $\lambda = 5710, 5529, 5527, 5183, 5172, 4481, 4456$ , which he ascribes to helium (or  $b$

as it occurs in hydrogen) existing as a component of magnesium without condensation. The proof is that  $\frac{2}{3}\lambda$ , which may be written  $\lambda'$ , satisfies the criteria above given for such lines; that is to say,  $\frac{4}{5}\lambda'$  is the wave-length of a line of the water-spectrum, as are also  $\frac{25}{32} \times \frac{46}{41}\lambda'$  and  $\frac{21}{32} \times \frac{70}{59}\lambda'$ . In fact, of the 21 numbers calculated from these formulæ, 15 agree, within a fraction of a unit of Ångström's scale, with the wave-lengths of lines of the water-spectrum observed by Liveing and Dewar. The remaining 6 lines the author thinks must have escaped observation merely through their febleness, because the wave-length of each of them is the half of the wave-length of a corresponding line in the compound line-spectrum of hydrogen; and he has elsewhere shown that, in general, the half of the wave-length of a line in the last-named spectrum is the wave-length of a line of the water-spectrum.

$\lambda'$  ought to satisfy also the criteria IV., but no line of wave-length  $\lambda'$  has as yet been observed in the spectrum of hydrogen; while of  $\frac{46}{41}\lambda'$  only 4135.5, and of  $\frac{70}{59}\lambda'$  only 4371.7, are the wave-lengths of observed lines of oxygen.

The magnesium-lines 5529, 4481, and 4456 coincide, within the limits of errors of observation, with lines of the compound line-spectrum of hydrogen.

The second group of magnesium-lines consists of those for which  $\lambda =$

5167	4481	3894
4808	4456	3892
4705	4351.2	3730
4703	4350	3334
4587	4165	3330
4586	4057.3	3329.1
4570	3896	3327

This group the author ascribes to the primary element *c* in the same state in which it occurs in oxygen; and it satisfies the criteria III., for in most cases  $\frac{3}{5}\lambda$  and  $\frac{5}{8}\lambda$  are found to agree, within a unit of Ångström's scale, with the wave-lengths of lines of the water-spectrum observed by Liveing and Dewar, and in most of the few remaining cases they are the halves of the wave-lengths of lines in the compound line-spectrum of hydrogen. The author gives a list of 43 lines of the oxygen-spectrum which belong to *c* in free oxygen; but of these only 4705, 4588, and 4455 are identical, or nearly so, with lines in the second group of magnesium-lines. In explanation of this, he says that when an element *c* occurs in different substances (as, for example, in oxygen, in magnesium,

and in carbon) in the same chemical state in each of them, but in combination with different kinds of matter, the intensities of the rays due to *c* will be variously affected by the action of these different kinds of matter. This is the reason why, for instance, many rays of *c* are seen in the oxygen-spectrum which disappear from the spectrum of magnesium or carbon and *vice versa*; so that without mathematical analysis it is almost impossible to recognize this element and trace it spectroscopically in different substances, even when it occurs in them in the same state. Lately the author has succeeded in detecting *c* in nitrogen also, and in the same state as it occurs in oxygen, carbon, and magnesium.

The third group of magnesium-lines consists of all the lines and sharp edges of bands, thirty in number, with wave-lengths between 3896 and 3329.1 observed by Liveing and Dewar and by Hartley and Adeney, and, according to our author, forms part of the large group of lines in the hydrogen-spectrum produced by the primary element *b* in the chemical state in which it occurs in hydrogen under the influence of *a* or "coronium." The wave-lengths of this group, when multiplied by  $\frac{4}{5}$ , give the wave-lengths of corresponding lines of the water-spectrum due to *b* as it occurs in hydrogen in water; and when multiplied by  $\frac{46}{41}$  and by  $\frac{70}{59}$ , give the wave-lengths of lines of oxygen due to *b* as it occurs in *O'* in oxygen and as it occurs in *O''* in *O'* in oxygen, respectively; and when multiplied by  $\frac{23}{32} \times \frac{46}{41}$  and  $\frac{21}{32} \times \frac{70}{59}$ , give the wave-lengths of corresponding groups of lines in the water-spectrum. None of the group have as yet been observed in the spectrum of hydrogen, but several of them have been observed in that of the oxyhydrogen-flame. As regards the water-spectrum, the agreement of the observed with the calculated lines is close and very complete throughout the long list of some ninety lines. As regards the oxygen-spectrum, only a few of the calculated wave-lengths correspond to lines of which observations are recorded. The existence of these, hitherto unnoticed, oxygen rays is therefore an inference from the existence of the rays of water which are harmonically related to them.

The fourth group of magnesium-lines includes 3336.2, and all the lines more refrangible than it observed by Liveing and Dewar and by Hartley and Adeney. This group forms part of the large group of lines of the water-spectrum due to *b* as it occurs in hydrogen in water-vapour; and for nearly every one of the fifty lines of the group, a line with the same wave-length within one unit of Ångström's scale has been observed by Liveing and Dewar. What differences of

wave-length there are may mostly be ascribed to errors of observation ; though they may be in some part due to slight, merely physical differences of condensation of the chemically condensed gas  $b$  as it exists in water-vapour on the one hand, and in magnesium on the other.

That these magnesium-lines, which are also water-lines, really belong to the group of lines due to  $b$  as it exists in hydrogen in water the author thinks to be proved ; because when their wave-lengths are multiplied by 5.4, they satisfy the criteria II. so far that if  $\lambda'' = 5.4 \lambda$ ,  $2\frac{3}{32} \times 46.41 \lambda''$  and  $21.32 \times 70.59 \lambda''$  respectively are groups of wave-lengths which agree well with observed wave-lengths in the water-spectrum.  $46.41 \lambda''$  and  $70.59 \lambda''$  should also be the wave-lengths of oxygen-lines ; but of these only a few agree with the wave-lengths of observed lines. The absence of the greater part of the lines of these groups from the observed oxygen-spectrum and of the group with wave-length  $\lambda''$  from the hydrogen-spectrum, the author ascribes to the weakness of the lines under ordinary circumstances ; and he expects that oxygen and hydrogen will yet, under some different conditions, be made to emit them with sensible intensity. His theory, then, is that magnesium is a compound containing :—

(1) Helium, without condensation or dilatation, which in magnesium emits only the rays of group I. ; all its other rays, including  $D_3$ , being weakened by the influence of the other components.

(2) The primary element  $c$ , in the same state in which it occurs in oxygen and carbon, giving the rays of group II.

(3) The primary element  $b$  in that state in which it occurs in free hydrogen, giving the rays of group III.

(4) The primary element  $b$ , but in the chemically more condensed state in which it occurs in the hydrogen of water-vapour, giving, under the influence of the other components, the rays of the partial group IV. with more or less intensity.

Applying the same method to carbon, he finds 11:1:1 rays of the elementary line-spectrum of that substance form five groups. The first consists of the rays for which  $\lambda$  is 6583.0, 6577.5, 5694.1, 5660.9, 5646.5, 5638.6, and 5150.5. In this group  $\lambda$ , when multiplied by 3.5, satisfies very completely the criteria II. for  $b$  as it occurs in hydrogen, so far as the calculated wave-lengths of rays of water are concerned ; but only one of the corresponding rays of hydrogen, and none of those of oxygen, have been recorded as occurring in the spectra of these substances. Nevertheless several of these rays have been observed by Liveing in the oxyhydrogen-



flame ; and in such a flame there are variable quantities of the uncombined gases, so that the rays in question may really be emitted by oxygen or hydrogen, through mixed up in the photograph with the rays of water. The ratio  $3/5$ , which expresses the relation between  $\lambda$  in this group to the wave-length of a corresponding ray in the spectrum of hydrogen, according to the author's theory indicates the ratio between the volume which a given quantity of  $b$  in free hydrogen occupies and the volume which the same quantity of  $b$  in carbon occupies. So that if the chemical density of  $b$  in free hydrogen be 1, that of  $b$  in the state in which it occurs in carbon will be  $5/3$ .

The second group of carbon-lines, for which  $\lambda$  is 6577·5, 5638·6, 5379·0, 5150·5, 5144·2, 5133·0, 4266·0, 3919, fulfils criteria III. for the element  $c$  in the same state as that in which it occurs in oxygen. For  $3/5\lambda$  and  $5/8\lambda$  agree closely with the observed wave-lengths of lines of the water-spectrum. This group, like the second group of magnesium-rays, forms but a part of the whole group of rays which  $c$  can emit when in the state of chemical condensation in which it occurs in oxygen and in magnesium. And this partial appearance of the group depends on the intensities which are determined by the influence of the other elements with which  $c$  is combined.

The third group of carbon rays, for which  $\lambda$  is 3919, 3881·9, 3877, 3875·7, 3870·7, 3589·9, 3584·8, 3583·3, 3167·7, 2993·1, forms, like the third group of magnesium-lines, a part of the group of lines which  $b$  can emit when it is in the state in which it exists in free hydrogen. It satisfies the same tests as the corresponding magnesium group and with equal completeness. That  $b$  in carbon should give an entirely different group of rays from that given by the same matter existing in the same state of condensation in magnesium, the author explains, as before, by the variations of intensity induced by the other elements in the two substances.

The lines of the fourth group, for which  $\lambda$  is 3166, 2993·1, 2967·3, 2881·1, 2837·3, 2836·7, 2835·9, 2746·5, 2733·2, 2640, and 2523·6 (silicon-line), like the fourth group of magnesium-lines, are also, within the limits of error of observation, lines of the water-spectrum. Also  $5/4\lambda$  satisfies, to the same extent as in the case of the corresponding group belonging to magnesium, the criteria II. for the wave-lengths of rays due to  $b$  in the same chemical state as that in which it occurs in the hydrogen in water-vapour ; and, as before, the author thinks that the ratio  $5/4$  indicates the condensation of  $b$ , in the form in which it exists in carbon and produces this group, as compared with its condensation in free hydrogen.

The fifth group consists of the remaining lines of carbon, together with the lines which were at first attributed to carbon by Liveing and Dewar, and were afterwards shown by Hartley to be produced by silicon. The author's analysis leads him to think that the latter lines belong both to carbon and silicon, and are due to a common element in those substances. The wave-lengths of the whole group, when multiplied by  $5/4$ , become wave-lengths of lines of the water-spectrum; and when multiplied by  $25/16$  give a group of wave-lengths which satisfies the criteria II. for  $b$  as it occurs in hydrogen as completely as any of the preceding groups so far as the water-spectrum is concerned, and somewhat more completely than them so far as the observed lines of hydrogen and oxygen are concerned. The factors  $5/4$  and  $25/16$  are interpreted to indicate the condensation of  $b$ , as in former cases. It is noteworthy that  $25/16 \times 2506.6 = 3916.5 = 2/3 \times 5874.7$ , the wave-length of the helium-ray  $D_3$ .

The conclusion is that carbon contains  $c$  in the same state as that in which it occurs in oxygen and in magnesium, and  $b$  in four different states: (1) in which it is less condensed than in hydrogen in the ratio  $3:5$ ; (2) in which it is in the same state as in free hydrogen; (3) in which it is in the state in which it occurs in the hydrogen in water-vapour, more condensed, in the ratio  $5:4$ , than in free hydrogen; and (4) in which it is still more condensed, in the ratio  $25:16$ , than in free hydrogen. In the second and third of these states  $b$  occurs also in magnesium.

XLVII. *The Absolute Wave-length of Light.*—Part II. By LOUIS BELL, *Fellow in Physics in Johns Hopkins University*.\*

[Continued from p. 263.]

THIS continuation of my previous paper contains the angular measurements and the details of the measurement and calibration of the gratings, together with the final results. In addition I have endeavoured to point out the probable sources of error in some recent determinations of absolute wave-length.

#### *Angular Measurements.*

In my former paper (this Journal, March 1887) the work with glass gratings was described in detail, so that it will only be necessary to summarize it here.

\* Communicated by the Author.

Grating I. was used during October and November 1886, and forty-eight series of observations were obtained as follows, each series consisting of three to seven observations:—

Date.	Number of series.	Angle.		
Oct. 19.	1	45°	1'	47.2"
20.	1	45	1	48.4
22.	2	45	1	48.2
23.	1	45	1	49.8
26.	4	45	1	49.3
27.	3	45	1	48.2
31.	1	45	1	50.1
Nov. 3.	1	45	1	48.6
4.	3	45	1	47.4
5.	2	45	1	47.9
10.	4	45	1	47.8
11.	6	45	1	49.7
16.	8	45	1	48.2
17.	5	45	1	47.5
20.	6	45	1	47.5

Grating I. was used at an average temperature of very nearly 20°, to which all observations were reduced. The average barometric height was 761 millim., so that no correction was required for this cause. Weighting and combining the above observations, the final value is

$$\phi = 45^{\circ} 1' 48''.24 \pm 0''.11,$$

corresponding to the spectrum of the third order.

The resulting probable error in wave-length is about one part in a million.

Grating II. was used in March 1887, at an average temperature of very nearly 20°, and an average pressure of 760 millim. Thirty-six series of observations were obtained in the fourth order, as follows:—

Date.	Number of series.	Angle.		
Mar. 6.	2	42°	5'	1.2"
10.	1	42	4	58.6
11.	7	42	5	1.4
15.	1	42	5	4.0
16.	6	42	4	57.8
17.	6	42	4	58.5
18.	7	42	4	59.1
23.	6	42	4	58.3

Combining and weighting, the mean value is

$$\phi = 42^{\circ} 4' 59''.28 \pm 0''.2.$$

The probable error is equivalent to about one part in six hundred thousand in the wave-length.

Both the glass gratings were used exclusively for the line  $D_1$ , which was on the whole most convenient for measurement,  $D_2$  being rejected by reason of the troublesome atmospheric lines. The relative wave-lengths of a very large number of lines have been so exactly determined by Prof. Rowland that any one of them would have given results equally valuable, and in the subsequent work with gratings III. and IV., two of these standard lines were employed.

In this second part of the investigation, the gratings as before mentioned were used on the large spectrometer in which the telescopes were kept at a fixed angle and the grating was turned. This method is, of course, applicable only to very solid instruments in which the angle can readily be kept constant, and it should be further noted that it also requires the use of very perfect gratings, since the grating is used asymmetrically. As a result of this the spectra on the two sides differ in dispersion; and if the ruling is irregular either in spacing or in contour of the individual lines, may differ quite widely in focal-length definition and illumination. After critical examination gratings III. and IV. appeared to be so nearly perfect in ruling as to be quite secure from the dangers of the method. The method has moreover the distinct advantage of enabling the angle of deviation to be varied within certain narrow limits. Hence it becomes possible so to arrange the apparatus as to give to some convenient line a double deflection that shall be an exact submultiple of  $360^{\circ}$ . This once accomplished it becomes an easy matter completely to eliminate the errors of the divided circle and obtain a value of  $n\phi$  dependent only on the micrometer constants, which in turn may be themselves almost eliminated. To be sure, this method practically confines observations to the spectra of a given order and limits the choice of lines for measurement, but the first objection does not apply to gratings of which the ruling is very nearly perfect, and since the relative wave-lengths of a large number of lines are known with very great exactness, measurements of the absolute wave-length are quite comparable even if made on different lines.

As regards the constancy of the angle between the collimator and observing telescope, there was every reason to expect entire permanence throughout the experiments; and observation soon justified this expectation. The telescopes



were firmly secured at both ends to one and the same casting, which in turn was firmly bedded in a brick pier. In addition the size of the apparatus was such that a variation of even  $1''$  in the angle was quite improbable. The angle, measured in the ordinary way with a collimating eye-piece, could be determined to  $1''$  of arc, exclusive of errors of graduation in the circle. At first there appeared to be distinct variations in the angle as determined at the beginning of each series of observations, reaching sometimes more than  $10''$ . It soon appeared, however, that when the same part of the circle was used the angle between the telescopes was sensibly the same, and the apparent variations were then traced to a periodic error in the divided circle, which by the method of repetition was completely eliminated from the measurements of angles of deviation, and only appeared in the determinations of  $\theta$ . This error was finally eliminated by measuring  $\theta$  in various portions of the circle.

The method of determining  $\phi$  was as follows:—The instrument being adjusted by the ordinary methods, a suitable line was selected for measurement, and then the angle  $\theta$  was slightly increased or diminished until, by measurement of a double deflection,  $n\phi$  was found to be very close indeed to  $360^\circ$ . Then a double deflection was carefully measured, and if time permitted several times repeated, an observer always being at the eye-piece to see that the line did not move from the cross hairs while the micrometers were being read. Then, clamping the main circle, the grating-holder was turned through  $2\phi$  until the line was very closely upon the cross hairs, any slight readjustments made necessary by this disturbance of the instrument were made, and the process was repeated. In this way the initial line of the circle was finally reached and a value of  $n\phi$  obtained which depended only on the algebraical sum of the micrometer-readings, always a small quantity.

The determination of the temperature, a very difficult and uncertain matter in the case of glass gratings, is here comparatively simple. A sensitive thermometer (Baudin 6156) was kept in contact with the grating, its bulb being carefully shielded by cotton. The construction of the spectrometer made it impracticable to shield the grating effectively from radiation from the observer's body; but the thermometer apparently proved effective in giving the real temperature, since no discrepancies in the results could be traced to thermal causes. The thermometer-readings were made to  $0^\circ.05$ , and the temperature of observation rarely varied more than two or three degrees from  $20^\circ$  C.

The temperature being thus obtained, the necessary correction was introduced directly into the angle of deviation. Writing the formula for wave-length in the form

$$\lambda = Cs \sin \phi,$$

where  $C$  is a factor depending on the method in which the grating is used, and differentiating we obtain

$$\frac{\delta s}{s} = -\cot \phi \delta \phi,$$

where, if we take  $1^\circ$  for the temperature variation,  $\frac{\delta s}{s}$  is the coefficient of expansion. Whence

$$\delta \phi = \frac{\frac{\delta s}{s}}{\cot \phi} =$$

correction for  $1^\circ$  variation in temperature. For grating III., for instance,  $\delta \phi = 2''.688$ ; and by this means all the deviations were reduced to  $20^\circ$ . Writing again the equation for wave-length in the form for the method here used,

$$c = \sin \phi \cos \theta.$$

Now to obtain the variation in  $\phi$  due to a change in the angle between the telescopes,

$$\delta \phi = \tan \phi \tan \theta \delta \theta.$$

Taking now  $\delta \theta = 1''$  and  $\phi$  as found in these experiments,

$$\delta \phi = 0''.089.$$

By this means the necessary correction could be introduced in the angle of deviation, but the angle between the telescopes was so nearly constant as to render this correction needless.

The line selected for measurement with III. was a sharp one in the green at 5133.95 of Rowland's map. The angle  $\theta$  between the telescopes was adjusted so that in the eighth order the double deflection was  $72^\circ$ . Eighteen complete series of observations were then obtained, each giving a value of  $10\phi$ , from which the errors of the circle were completely eliminated. The results in detail were as follows, corrected to  $20^\circ$  on thermometer used:—

Date.		$\phi$ .
1887. Nov. 2	. . .	$36^{\circ} 0' 27''.19$
	3 . . .	$36 0 25.87$
	4 . . .	$36 0 24.40$
	5 . . .	$36 0 24.95$
	5 . . .	$36 0 26.83$
	9 . . .	$36 0 26.14$
	16 . . .	$36 0 27.40$
	16 . . .	$36 0 27.37$
	17 . . .	$36 0 27.57$
	22 . . .	$36 0 25.16$
	29 . . .	$36 0 25.69$
	29 . . .	$36 0 25.99$
	29 . . .	$36 0 25.91$
	30 . . .	$36 0 26.10$
	30 . . .	$36 0 25.86$
	30 . . .	$36 0 25.81$
Dec. 1	. . .	$36 0 25.68$
	1 . . .	$36 0 25.80$

The last decimal place is retained simply for convenience in averaging. The mean value of  $\phi$  is  $36^{\circ} 0' 26''.07$ , which, reduced for the error of thermometer at  $20^{\circ}$ , gives finally

$$\phi = 36^{\circ} 0' 25''.17.$$

The probable error of this value is  $0''.14$ . The effect of a small error in  $\phi$  on the resulting wave-length is given at once by

$$\delta\lambda = \cos \phi \delta\phi.$$

In this case the error introduced by an error of  $1''$  in  $\phi$  is a little less than 1 part in 250000.

The mean value of  $\theta$  during these measurements was

$$\theta = 6^{\circ} 59' 58''.56.$$

In case of grating IV. the line selected for observation was one of Rowland's standards at w.l. 5914.319 of his preliminary list. It is a very close double, the components being distant from each other something like  $\frac{1}{75000}$  of their wave-length. The double deflection was as before  $72^{\circ}$ , but in the fifth order. As with grating III. eighteen series of observations were obtained, with the following resulting values of  $\phi$  :—

Date.		$\phi$ .	
1887.	Dec. 16	. . .	$36^{\circ} 0' + 1''16$
	16	. . .	$36 0 + 0.66$
	16	. . .	$36 0 + 0.67$
	19	. . .	$36 0 + 0.64$
	19	. . .	$36 0 + 1.56$
	19	. . .	$36 0 + 0.85$
1888.	Jan. 12	. . .	$36 0 - 1.19$
	12	. . .	$36 0 - 1.61$
	12	. . .	$36 0 - 1.79$
	14	. . .	$36 0 - 1.09$
	14	. . .	$36 0 - 0.95$
	14	. . .	$36 0 - 0.89$
	19	. . .	$36 0 - 0.48$
	19	. . .	$36 0 - 0.59$
	19	. . .	$36 0 - 0.49$
	20	. . .	$36 0 + 0.51$
	20	. . .	$36 0 - 0.11$
	20	. . .	$36 0 + 0.55$

The mean value, corrected as before for error of thermometer, is

$$\phi = 35^{\circ} 59' 59''.06 + 0''.15.$$

The effect of this probable error is obviously the same as in the case of grating III. The mean value of the semiangle between the telescopes was

$$\theta = 6^{\circ} 58' 31''.0.$$

During the observations with grating III. the barometric height reduced to the place of observation was very nearly 762 millim., but during the work with grating IV. it was phenomenally high, reaching an average value of 766 millim., an amount so far from normal pressure as to render a small correction necessary.

The mean temperature during the observations with III. was about  $21^{\circ} \text{C}$ ., but in the case of IV. it averaged almost exactly  $20^{\circ} \text{C}$ ., varying at most only two or three degrees from that figure.

#### *Measurement of the Gratings.*

The comparator on which this, the most important portion of the research, was accomplished was the same one described in my previous paper. It had, however, been improved in several particulars. The platform carrying the standards had been fitted with smooth rack-and-screw adjustments, and the microscopes and micrometers were new. The illumination of a grating under the power used,—two hundred and fifty



diameters—is by no means an easy matter, and at the same time a powerful and symmetrical illumination is absolutely necessary for the most accurate work, particularly in the case of rather small grating-spaces. I had been thoroughly dissatisfied with the illumination previously used—a lamp at a suitable distance—and now made a radical change. A three-candle power electric lamp was attached directly to the microscope just below the eyepiece and about a foot above the objects measured. A small mirror carried by an arm screwed to the objective reflected the beam into the Tolles illuminator. A glass bulb filled with water surrounded the light and served the double purpose of stopping radiation and partially condensing the beam upon the mirror above mentioned.

I am aware that such an arrangement is somewhat revolutionary, and it was only after a careful trial that I convinced myself that the heat from so near a source was not injurious.

In the first place, it should be noted that the lamp is only used for a few moments at a time and at intervals long compared with the time of observation. Thus the very minute heat-wave that reaches the bar through the bulb of water cannot possibly produce a perceptible rise of temperature during the time of an observation, while during the intervals it is completely dissipated.

As an experimental fact, no heating effect whatever is sensible even after a whole day's observations. To show at once this fact, and the general character of an average series of comparisons I subjoin ten comparisons of  $\text{Dm}_1\text{S}^{\text{a}}_2$  with a certain decimetre on glass, made at intervals of about three-quarters of an hour on two successive days. The figures are taken directly from my note-book.

Date.	$\delta$ .	T=
June 1, 1887 . . .	$\text{Dm}_1\text{S}^{\text{a}}_2 = \text{G} + 21\cdot3$	$17\cdot4$
" . . .	+ 21·6	17·4
" . . .	+ 22·1	17·5
" . . .	+ 22·1	17·5
" . . .	+ 20·8	17·5
" . . .	+ 20·1	17·5
June 2, 1887 . . .	+ 21·4	17·0
" . . .	+ 21·0	17·0
" . . .	+ 21·0	17·0
" . . .	+ 21·0	17·1

The temperature was given by a thermometer in contact with  $\text{S}^{\text{a}}_2$ , and 18 of the micrometer equalled  $0^{\circ}\cdot28$ . In a comparison of two standards with such unequal coefficients of expansion as glass and speculum metal, the evil effects of

radiation should be at their maximum; but the preceding series, including as it does all the experimental errors, and showing an extreme variation of but  $0^{\circ}5$  leaves, I think, little to be desired.

The comparator was placed in a vault some six feet below the level of the street, which was provided with thick double walls with an air-space between. This observing-room enabled the temperature to be kept down to a daily variation of less than half a degree, the extreme range for several days being frequently less than that amount. Before this vault in the new Physical Laboratory was completed, the comparator had been placed in an upper room of one of the old buildings, where it was well nigh impossible to keep anything like a constant temperature, particularly since the heat was unavoidably partially shut off during the night. Owing to this state of affairs the measurement of the gratings on which my preliminary wave-length was based was made under difficulties, and in most of the series necessarily under a rising temperature. Now when a glass standard is measured against a metal one, glass, being a notoriously bad conductor, and having a very small coefficient of expansion, if any rise of temperature takes place the length found for the glass will be too small, for, responding less readily to a change, it will be actually measured at a lower temperature.

It therefore became necessary to re-measure the glass gratings Nos. I. and II., to eliminate this source of error, which was done before the results for III. and IV. were obtained. These gratings are very nearly 3 centim. long, and they were therefore compared with successive triple centimetres of  $S^a_2$  until the fifteen-centimetre mark was reached. Grating I. was first taken in hand and six complete series of observations were obtained, each micrometer reading being the mean of several, and the extreme limits of temperature-variation during the two days occupied by the comparisons being  $0^{\circ}3$  C. The following gives a summary of the results:—

$$\left. \begin{array}{l} \text{cm.} \qquad \delta. \\ 5G = 15 S^a_2 + 19.0 \\ 5G = 15 S^a_2 + 21.5 \\ 5G = 15 S^a_2 + 18.1 \\ 5G = 15 S^a_2 + 23.9 \\ 5G = 15 S^a_2 + 22.6 \\ 5G = 15 S^a_2 + 18.3 \end{array} \right\} \text{At } 19^{\circ}9 \text{ C.}$$

Hence combining these and reducing them to the standard temperature of  $20^{\circ}$  we have:—

$$60,000 \text{ spaces} = 5G = 15 \text{ centim. } S^a_2 + 5^{\mu}2 \text{ at } 20^{\circ}.$$

The micrometer-constant here used was that of the new micrometer, where  $1\delta = 0^{\mu}.257$ .

In precisely the same way grating II. was remeasured, the six series giving the following relations :—

$$\left. \begin{array}{l} 5G = 15 S_2^a + 157.4 \\ 5G = 15 S_2^a + 154.9 \\ 5G = 15 S_2^a + 154.5 \\ 5G = 15 S_2^a + 152.4 \\ 5G = 15 S_2^a + 154.9 \\ 5G = 15 S_2^a + 162.4 \end{array} \right\} \text{At } 19^{\circ}.8 \text{ C.}$$

Combining and reducing these results as before, we have the equation

$$42640 \text{ spaces} = 5G = 15 \text{ centim. } S_2^a + 39^{\mu}.9 \text{ at } 20^{\circ}.$$

The temperature-variation in the two days of observation was only  $0^{\circ}.2$ .

Gratings III. and IV. were then measured. In this case a large number of comparisons were obtained at both high and low temperatures with the object of detecting any differences which might exist between the coefficients of expansion of the gratings and those of the speculum-metal standards. III. and IV. being a little over a decimetre in length were very easy to measure, particularly since the lines were very sharp and of approximately the same width as those on the standards.

III. proved to have sensibly the same coefficient as the standards. I subjoin the comparisons made at or very near  $20^{\circ}$ :—

$$\begin{array}{rcl} & \delta. & \\ G = Dm_1 S_2^a + 32.9 & & \\ G = & \text{,,} & + 33.0 \\ G = & \text{,,} & + 32.7 \\ G = & \text{,,} & + 33.2 \\ G = & \text{,,} & + 32.3 \\ G = & \text{,,} & + 32.6 \\ G = & \text{,,} & + 34.5 \\ G = & \text{,,} & + 33.4 \\ G = & \text{,,} & + 34.2 \\ G = & \text{,,} & + 32.6 \end{array}$$

Combining these and other series of observations gives finally

$$28418 \text{ spaces} = G = Dm_1 S_2^a + 8^{\mu}.5 \text{ at } 20^{\circ}.$$

It should be noted that the extreme variation in the above

series is  $2^{\delta} \cdot 2$ , very nearly  $0^{\mu} \cdot 5$ , or one part in two hundred thousand.

In the case of IV. the coefficient appeared to be somewhat smaller than that of  $S^a_2$ . The range of temperature secured was not large, but as nearly as could be ascertained the coefficient is about  $16^{\mu} \cdot 1$  per metre per degree, while that of the standards is  $17^{\mu} \cdot 9$  per metre per degree. However, since the measurements of  $\phi$  made with IV. were distributed with a tolerable degree of symmetry on both sides of  $20^{\circ}$ , any error due to an inexact value of the coefficient of expansion would appear mainly in the probable error of  $\phi$ . The variation found would, as a matter of fact, have changed the final value of  $\phi$  by less than  $0'' \cdot 2$ .

The comparisons of IV. made near  $20^{\circ}$  were as follows:—

	$\delta$ .
$G = Dm_1 S^a_2 + 35 \cdot 8$	
$G = \quad \quad \quad + 35 \cdot 5$	
$G = \quad \quad \quad + 35 \cdot 6$	
$G = \quad \quad \quad + 36 \cdot 0$	
$G = \quad \quad \quad + 34 \cdot 0$	
$G = \quad \quad \quad + 35 \cdot 8$	
$G = \quad \quad \quad + 34 \cdot 6$	
$G = \quad \quad \quad + 36 \cdot 3$	
$G = \quad \quad \quad + 33 \cdot 3$	
$G = \quad \quad \quad + 36 \cdot 7$	

Combining these and the other observations,

$$39,465 \text{ spaces} = G = Dm_1 S^a_2 + 9^{\mu} \cdot 1 \text{ at } 20^{\circ}.$$

The probable error of the relations found for III. and IV. can hardly exceed one part in a million so far as the distance between the terminal lines selected is concerned. These terminal lines were varied at each comparison, so that while each of the above relations represents 39,465 spaces, the lines measured between, though in the same vicinity, are seldom or never identical.

In gratings I., II., III. the number of spaces was very easily counted, as the dividing-engine automatically rules every hundredth line longer, and every fiftieth line shorter, than the others. In grating IV. the number of spaces was found readily enough by ruling at a known temperature the terminal lines of a test-plate almost exactly a decimetre long, and containing a known number of lines. A comparison of this with the grating gave the quantity required.

#### *Calibration of the Gratings.*

In my previous paper the need and method of determining



the errors of ruling in a grating were briefly noticed. It will be well here to enter somewhat more into detail.

The grating-space is never perfectly uniform throughout the whole extent of the ruled surface. The variations may be in general classed as regular and irregular. In the first class we put variations in the grating-space which are purely periodic or purely linear. These produce respectively "ghosts" and difference in focus of the spectra on opposite sides of the normal. Either fault might be large enough to unfit the grating for wave-length determination, and would always be undesirable, but nevertheless would introduce no gross errors into the result. Variations of the second class include the displacement, omission or exaggeration of a line or lines, and, what is of great importance, a more or less sudden change in the grating-space producing a section of the grating having a grating-space peculiar to itself. The former types of accidental error, unless extensive, are harmless, and are present in most gratings, usually showing as faint streaks in the ruling. It is with the last-mentioned error that we mainly have to do.

Consider a grating the spacing of which is sensibly uniform except throughout a certain portion. Let that portion have a grating-space distinctly larger or smaller than that of the remainder of the grating. If the abnormal portion is a considerable fractional part of the whole grating it will, in general, produce false lines and injure or ruin the definition of the grating. Such a grating we should now throw aside as useless, although many of the older gratings are thus affected. Suppose, however, that the abnormal portion is confined to a few hundred lines. Such a series of lines will have little brilliancy and less defining-power, and consequently will simply diffuse a certain amount of light without either producing false lines or, in general, injuring the definition. In short, when the full aperture of the grating is used, the spectra produced will be due only to the normal grating-space, the abnormal portion having little or no visible effect. If, however, we attempt to evaluate the grating-space by measuring the total length of the ruled surface and dividing it by the number of spaces therein contained, we shall obtain an incorrect result, since this average grating-space, including, as it does, the abnormal portion, will be necessarily different from the normal grating-space which produces the spectra observed.

In general, if  $n$  be the total number of spaces and  $s$  the normal grating-space, the length of the ruled surface will be  $ns + A$ , where  $A$  is a quantity depending on the magnitude

and nature of the abnormal portion. It will have for its maximum value  $\Sigma(s-s')$ , where  $s'$  is the varying grating-space, in the case when the change in the spacing is so local and sudden as to produce no effect at all on the spectrum; and will be variously modified by the considerations now to be mentioned. If we could always assume that the abnormal portion of the grating produced no effect on the spectrum, the elimination of errors of ruling would thus become comparatively simple. But in practice it is not very uncommon to find gratings in which there are several portions where the spacing is abnormal, in one case perhaps producing no effect, in a second producing false lines, and in a third causing a faint shading off of the lines. For an abnormal portion will produce no effect, a slight shading, or reduplicated lines, according to its extent and the amount of its variation from the normal.

The following experiment will readily show the laws which govern these errors of ruling. Place a rather bad grating—unfortunately only too easily obtained—on the spectrometer, and setting the cross-hairs carefully on a prominent line, gradually cover the grating with a bit of paper, slowly moving it along from one end. In very few cases will the line stay upon the cross-hairs. A typical succession of changes in the spectrum is as follows: Perhaps no change is observed until two-thirds of the grating has been covered. Then a faint shading appears on one side of the line, grows stronger as more and more of the grating is covered, and finally is terminated by a faint line. Then this line grows stronger till the original line appears double and finally disappears, leaving the displaced line due to the abnormal grating-space. This description, I regret to say, is from the examination of a grating which had been used for the determination of absolute wave-lengths.\* This case is exceptionally complete, but even with a very good grating minute displacements can usually be noticed.

When the abnormal portion is sufficiently extensive to produce a faint shading along one side of the lines when the full aperture of the grating is used, the effect of the error on the resulting wave-length may be in part eliminated by the fact that the shading would displace the apparent centre of the line and hence slightly change the observed angle of deviation. For this reason a grating so affected would be likely to give results varying with the order of spectrum used, since the appearance of the line would vary somewhat with the illumination. It is at once apparent, however, that no combination

\* Not by the author it is almost needless to add.

of the results from different orders of spectra can possibly eliminate the class of errors we are discussing, since the algebraic sign of the error will be the same for all orders, and it will be in every case a nearly constant fraction of the wave-length.

The problem before the experimenter is then the following: To detect the existence and position of any abnormal portion of the grating in use, to separate as far as possible such portions as produce a visible effect from those which do not, and thus finally to determine the proper value to be assigned to the quantity  $A$ .

The investigation is somewhat simplified by the fact that, for the most part, abnormal spacing occurs at an end of the ruled surface, generally at the end where the ruling was begun, since, when the engine is started, it is likely to run for some little time before it settles down to a uniform state. Then, too, one is able to disregard the slight and gradual variations in the grating-space which appear in every grating, since their effects will in general be integrated in the spectrum produced.

It only remains, therefore, to study those larger and more sudden changes which can produce a sensible error in the result. It is evident that the process of examination indicated above will serve to detect the more extensive faults, together with any errors of figure in the surface, but an abnormal portion, consisting of only a few hundred lines, will not have resolving power enough to produce a marked effect. Making then a slit in a card just wide enough to expose a sufficient number of lines to give tolerable definition, one can examine the grating, section by section, and still further discriminate between the normal and abnormal spacing, errors of figure being included as before. But as the number of abnormal spaces decreases, a point will be reached when this method breaks down completely; and since the error in the resulting wave-length may be as large in this case as when the fault is more extended, another method must be sought. So far as I know, the only method which will detect and evaluate all these errors is that which I have called calibration, measuring the relative lengths of  $n$  grating-spaces taken successively along the ruled surface. The process employed was as follows: The stops of the comparator were set as close together as practicable, limiting the run of the carriage to a distance which varied in different cases from 4 to 10 millim. Then the grating to be examined was brought under the microscope, and micrometer-readings were taken on the lines just within the run of the carriage; the grating was then



moved along about the length of the run, and the process repeated till the whole grating had been gone over. The variations in the micrometer-readings then gave the variations in the length of  $n$  spaces in different parts of the grating. The only assumption involved was that the variation in the different sections did not amount to an entire space, an hypothesis quite secure in gratings with spaces as large as those employed. It was thus possible to determine quite accurately the variations in the grating-space throughout the whole grating.

It should be noted that, since these variations may be of almost any kind and magnitude, the errors produced by them will not in general be eliminated by combining the results obtained from several gratings. It may happen that the gratings used by one experimenter will have errors that will counterbalance each other, while those used by another will all have errors of the same sign. For instance, by the merest accident the gratings used by the writer gave nearly identical results corrected and uncorrected, while those used by Peirce uniformly required a reduction in the resulting wave-length. The number of gratings used by a given investigator is, however, so small that the errors will very seldom be eliminated; while no combination of the results obtained from different orders of the same grating can produce any useful effect whatever.

Each of the gratings used in this research was examined minutely by the above methods, and in each was found an abnormal portion of one sort or another. Of eight gratings which I have calibrated all have shown a similar error; and of more than twenty which I have examined in the spectrometer only one (grating III.) failed to show an abnormal section at one end. Since this is the commonest form of the error in question, it is but natural to inquire why it cannot be avoided by covering the defective end. The reason is simple enough. By stopping out the defective portion the grating is reduced to an incommensurable length, which enormously increases the difficulty of measuring it. A grating which is in length some convenient submultiple of a metre is easy to measure with a comparatively high degree of exactness: but one which is, say, twenty-seven millimetres long, is exceedingly difficult to measure accurately, since it involves a long micrometer run or the errors of subdivision down to single millimetres. It is, therefore, better to use the full aperture of the grating and find  $\lambda$  by calibration.

In calibrating the gratings used, I divided I. and II., which were thirty millimetres long, into six sections of 5 millim.,



and the large gratings III. and IV. into centimetres. Each grating was carefully gone over five times and the mean result taken. The following corrections were found.

The actual variations found in each grating are given below, the figures given being the difference of  $n$  lines from the distance between the stops, the lines being taken in the consecutive sections of the gratings.

## Grating I.

	Sections	1	2	3	4	5	6
Residuals . . . . .		0 $\mu$ .78	0.98	0.81	1.03	0.86	1.24

## Grating II.

	Sections	1	2	3	4	5	6
Residuals . . . . .		2 $\mu$ .07	1.93	1.52	1.68	1.31	0.45

## Grating III.

	Sections	1	2	3	4	5	6	7	8	9	10
Residuals . . . . .		2 $\mu$ .80	2.85	1.77	2.77	2.70	2.77	2.67	2.64	2.73	2.77

## Grating IV.

	Sections	1	2	3	4	5	6	7	8	9	10
Residuals . . . . .		0 $\mu$ .31	0.28	0.35	0.43	0.40	0.43	0.31	0.35	0.28	0.82

The calibration of III. is worth describing in detail. Centimetre 3 was evidently too long. I therefore measured the centimetres from 15 to 25 millim. and from 25 to 35 millim. The former was quite normal, but the latter showed an increase almost identical with that of the whole third centimetre. I then examined the grating in a strong light and detected at 27 millim. from the end a faint line, such as usually indicates a few wavering lines, caused perhaps by dust under the diamond point. Placing, however, this line under the microscope, a band of perhaps twenty lines appeared with spacing noticeably wider than usual. Here was a very serious flaw in a grating to all appearance absolutely perfect. A most critical examination in the spectrometer of course failed to detect it, but it was both detected and located with unerring certainty by the process of calibration. Micrometrical measurements on this group showed an excess of about 2 $\mu$ .5 over an equal number of spaces elsewhere on the grating. This quantity, of course, had to be taken account of in connexion with the previous calibration.

The deduction of the necessary corrections from the data given by calibration requires no little care and judgment, and can be properly done only in connexion with a detailed study of the spectra given by various portions of the gratings concerned. For the four gratings used by the author, these corrections, applied directly to the lengths of the gratings in

the form of the quantity A before mentioned, are very nearly as follows:—

Grating.	A.
I. . . . .	$-\overset{\mu}{0}\cdot10$
II. . . . .	$+0\cdot40$
III. . . . .	$-2\cdot00$
IV. . . . .	$+0\cdot45$

It should be distinctly understood that the corrections deduced from the calibration are necessarily only approximate. A very minute examination of a grating on the spectrometer is impossible, since a small section of the ruled surface has not sufficient resolving power to give measurable spectra. On the other hand, while calibration gives the variations of the grating-space with a high degree of exactness, it obviously cannot definitely decide how far these variations are integrated in the spectrum measured. Consequently, while calibration will in every case give a valuable approximation, it must necessarily leave residual errors.

In these experiments the gratings were always measured parallel to the terminations of the lines. Consequently the length of each grating as found directly must be multiplied by  $\cos(90^\circ - a)$ , where  $a$  is the angle made by an individual line with the line formed by the locus of the terminations. In the case of gratings I., II., III., this angle was found by measuring a test-plate as described in my previous paper, and was found to be within a very few seconds of  $89^\circ 56'$ .

Grating IV., ruled on the new engine, was tested by measuring the sides and diagonals of the ruled surface, and gave an almost exactly identical value of  $a$ . No correction, therefore, need be introduced for this cause, since  $\cos(90^\circ - a)$  does not differ sensibly from unity.

#### *Final Result for Absolute Wave-length.*

Only one equation needs to be added to those already given for  $Sa_2$ . This is the one for the third 5-centim. space, necessary to determine the absolute length of the first 15 centim. 5-centim., (3) and (4) were compared, and the following relation was found between them:  $(4) = (3) + 0^\mu\cdot4$ . The relation found in 1885 was  $(4) = (3) + 1^\mu\cdot1$ . Consequently (3) has not sensibly shortened, and nearly the whole change found in  $Sa_2$  has taken place in the last five centimetres. Writing now the absolute lengths of

$Dm_1Sa_2$  and 15-cm.  $Sa_2$ ,

$Dm_1Sa_2 = 100\cdot00666$  millim. at  $20^\circ$ .

15-cm.  $Sa_2 = 150\cdot00897$  „ at  $20^\circ$ .

Applying now the relations found for grating I. in the foregoing section,

$$s = 0.002500226 \text{ millim.}$$

And since  $\phi = 45^\circ 1' 48''.24,$   
 $\lambda = 5896.18.$

Similarly for grating II.,

$$s = 0.003519041 \text{ millim.,}$$

$$\phi = 42^\circ 4' 59''.28,$$

$$\lambda = 5896.23.$$

Computing the similar quantities for the speculum-metal gratings III. and IV., for grating III.,

$$s = 0.003519358 \text{ millim.,}$$

$$\phi = 36^\circ 0' 25''.17,$$

$$\theta = 6^\circ 59' 58''.56,$$

$$\lambda = 5133.89;$$

and for grating IV.,

$$s = 0.002534306 \text{ millim.,}$$

$$\phi = 35^\circ 59' 59''.06,$$

$$\theta = 6^\circ 58' 31''.0,$$

$$\lambda = 5914.37.$$

Reducing now these latter wave-lengths to the corresponding values of  $D_1$ , introducing the barometric corrections and combining, the final results for that line are :—

Grating.	W. L.
I. . . . .	5896.18
II. . . . .	5896.23
III. . . . .	5896.15
IV. . . . .	5896.17

Finally, then, the mean value of the absolute wave-length of  $D_1$  in terms of the mean value assigned to  $S_{a_2}$  is

$$5896.18$$

in air at 760 millim. pressure and  $20^\circ$  C. temperature, or, *in vacuo*,

$$5897.90.$$

It is no easy matter to form an estimate of the probable error of this final result. So far as errors of observation go, the result should be correct to within one part in half a million, but there are so many complex sources of constant errors in this problem that such a statement means little. My present result exceeds the estimated probable error of my former result considerably, though it falls within the limit set by Prof. Rowland and myself for the possible error, and noted in his paper on "Relative Wave-lengths" of the same date as

my own. The cause of this discrepancy is partly due to the varying temperature under which the glass gratings were first measured, and partly to the change in the value assigned to the standard of length.\*

Then, too, the corrections applied to gratings II. and III. may be slightly in error. Taking into account all these sources of uncertainty, it is my opinion that the above final result is not likely to be in error by an amount as great as one part in two hundred thousand.

Taking the above value of the absolute wave-length and applying the appropriate corrections to some of the fundamental lines given in Prof. Rowland's paper (this Journal, March 1887), the wave-lengths of the principal Fraunhofer lines in air at 20° and 760 millim. are,

A	{ line between " head " and " tail " of group }	. . . 7621.31
B	" . . . . .	6884.11
C	. . . . .	6563.07
{ D <sub>1</sub>	. . . . .	5896.18
{ D <sub>2</sub>	. . . . .	5890.22
{ E <sub>1</sub>	. . . . .	5270.52
{ E <sub>2</sub>	. . . . .	5269.84
b <sub>1</sub>	. . . . .	5183.82
F	. . . . .	4861.51

Comparisons between these wave-lengths and the older ones become somewhat uncertain toward the ends of the spectrum, since the appearance of lines like A, B, G, and H varies greatly with the dispersion employed. The relative wave-lengths from which the above values are calculated are certainly exact to within one part in half a million.

It may not be out of place here to discuss the most recent work on this problem. Just before the publication of my first paper the very elaborate paper of Müller and Kempf appeared. Their work is a monument of laborious research, and it is unfortunate that so much time should have been spent in experiments conducted with glass gratings of small size and inferior quality. Since the invention of the concave grating it is a waste of energy to make micrometric measurements with plane ones, and this statement could hardly be corroborated more strongly than by the relative

\* In terms of the length I originally assigned to S<sub>2</sub>, the wave-length of D<sub>1</sub> would be 5896.14, while if the value deduced from the Berlin comparison were taken it would be 5896.22. The wave-length quite certainly lies between these values, but the proper weight to be given to the Berlin comparison relatively to the others is rather uncertain.



wave-lengths given by Müller and Kempf. The probable error of their wave-lengths is in general not less than one part in two hundred thousand. That the value assigned by them to the absolute wave-length is as near the truth as it probably is, is not due to lack of faults in the gratings. Their results for the line  $D_1$  were as follows:—

Grating.	W.L.
"2151" . . . .	5896·46
"5001" . . . .	5896·14
"8001" . . . .	5895·97
"8001 L" . . . .	5896·33

A discussion of these errors as exemplified in the paper under consideration would take up too much space to be inserted here, but one or two points are worthy of notice. When a grating gives different results in the different orders, it is evident that there are in it serious errors of ruling, and the maximum amount of the variation will give a rough estimate of their size as compared with those of other gratings. Applying this test, the four gratings rank as follows:—"5001," "8001 L," "2151," "8001," where the first, which gave for the w.l. 5896·14, had no sensible variation in the different orders, and the last, which gave 5895·97, varied in the most erratic fashion. It by no means follows, however, that because a grating gives identical results in the various orders, it is therefore free from errors of ruling. Witness grating III. of this paper, in which the error was of a kind which could not be detected at all in the spectrometer. Yet it was large enough to give, if neglected, 5896·28 for the wave-length of  $D_1$ \*. Speaking of errors in gratings, a case in point is the work of Peirce. On account of the reasons heretofore noted, Peirce's standards of length are somewhat uncertain in value, so that from this cause no definite correction can be as yet applied to his wave-length. Three of his gratings, however, I have calibrated, and each of them showed an error tending to diminish the wave-length. If the mean result obtained from these had been assumed to be correct, it would have been equivalent to the introduction of a constant error. Peirce's preliminary result is for this reason too large by more than one part in a hundred thousand; how much more, it is impossible to say without knowing the results

\* The results given by the gratings used by the author, neglecting the correction A, would be as follows:—

I. 5896·20; II. 5896·14; III. 5896·28; IV. 5896·12.

Curiously enough the mean would be practically unchanged.

obtained from each grating, and so being able to apply the corrections found. Peirce's method was such as should have secured very excellent results, and such will undoubtedly follow a further investigation of the standards and gratings. Still another recent determination is that by Kurlbaum, who used two good-sized speculum-metal gratings and measured them with particular care. Like the previous experimenters, he neglected, although he did not ignore, the errors of ruling, and consequently the results he obtained are somewhat in doubt. A serious objection, moreover, to his work is the very small spectrometer he used. To undertake a determination of absolute wave-length with a spectrometer reading by verniers to  $10''$  only, and furnished with telescopes of only one-inch aperture is simply courting constant errors. More especially is this true, since it would be hard to devise a method more effective in introducing the errors of ruling than to use a grating with telescopes too small to utilize its full aperture, and then determine the grating-space by measuring the total length of the ruled surface. Kurlbaum's gratings, too, were of an unfortunate size, 42 and 43 millim. broad respectively, and consequently by no means easy to measure. On the whole his result,  $5895.90$ , is not surprising.

The agreement of relative wave-lengths as determined by different experimenters unfortunately gives no measure as to the accuracy of the work. The relative wave-lengths, as determined by Müller and Kempf and by Kurlbaum, agree in general to within 1 part in 100,000; the absolute wave-lengths assigned by these experimenters vary by more than 1 part in 30,000.

A very ingenious flank movement on the problem of absolute wave-length has been made by Macé de Lépinay. His plan was to use interference-fringes in getting the dimensions of a block of quartz in terms of the wave-length, and then to avoid the difficulties of the linear measurement by obtaining the volume through a specific-gravity determination. His results do not indicate, however, experimental accuracy as great as can be obtained by the usual method, and the final reduction unfortunately involves a quantity even more uncertain than the average standard of length, *i. e.* the ratio between the metre (?) and the litre.

It may be interesting here to collect the various values which have been given for the absolute wave-length within recent years. Results are for the line  $D_1$ .

Mascart . . . . .	5894.3
Van der Willigen . . . . .	5898.6
Ångström . . . . .	5895.13
Ditscheiner . . . . .	5897.4
Peirce . . . . .	5896.27
Ångström corrected by Thalén .	5895.89
Müller and Kempf . . . . .	5896.25
Macé de Lépinay . . . . .	5896.04
Kurlbaum . . . . .	5895.90
Bell . . . . .	5896.18

These figures are discordant enough. When beginning the present work, I had hoped that it would prove possible to make a determination of absolute wave-length commensurate in accuracy with the relative wave-lengths as measured by Prof. Rowland. This hope has proved in a measure illusory, by reason of the small residual errors of the gratings and the greater uncertainty involving the standards of length. I feel convinced, however, that the result reached is quite near the limit of accuracy of the method. It should be remembered that any and every method involves the uncertainty of the standards of length, an uncertainty not to be removed until a normal standard is finally adopted and exact copies of it distributed. And as far as experimental difficulties are concerned, the next order of approximation will involve a large number of small but troublesome corrections, such as the effect of aqueous vapour on atmospheric refraction, varying barometric height, the minute variations in the grating-space, failure of thermometer to give temperature of grating exactly, and countless others which will suggest themselves only too readily.

Aside from the use of gratings, decidedly the most hopeful method as yet suggested is that due to Michelson and Morley\*. Theoretically the plan is particularly simple and beautiful, consisting merely in counting off a definite number of interference-fringes by moving one of the interfering-mirrors and measuring, or laying off upon a bar, the resulting distance. The mechanical difficulties in the way are, however, formidable, and whether or no they can be surmounted only persistent trial can show. The possible sources of error are of much the same type and magnitude as those involved in the comparison of standards of length; and if these errors are avoided, the uncertainty concerning the standards still remains. Whether or no the practical errors of the method are greater

\* This Journal, Dec. 1887, p. 463.

or less than with gratings only experience can prove. Certainly, if the method is capable of giving exact results, it is in the hands of one able to obtain them from it.

In closing this paper I can only express my sincerest gratitude to the various friends who have done all in their power to facilitate my work, and especially to Professor W. A. Rogers, who has been tireless in his endeavours to determine the true value of the standards of length; to Mr. J. S. Ames, Fellow in this University, who has given me invaluable aid in the work with metal gratings; and to Professor Rowland, who has furnished all possible facilities and under whose guidance the entire work has been carried out.

Physical Laboratory,  
Johns Hopkins University,  
March 1888.

**XLVIII.** *The Temperature at which Nickel begins to lose suddenly its Magnetic Properties.* By HERBERT TOMLINSON, B.A.\*

IT has long been known that nickel, like iron, begins at a certain temperature to rapidly lose its magnetic properties, and that the critical temperature for the former metal is much lower than for the latter. According to Faraday, nickel loses its magnetic permeability about  $330^{\circ}$  to  $340^{\circ}$  C.; according to Becquerel, about  $400^{\circ}$  C.; according to Pouillet, about  $350^{\circ}$  C.; and according to Chrystal, about  $400^{\circ}$  C. Berson, however, seems to have been the first† to publish a curve showing the relation between magnetic induction and temperature right up to the point at which the former ceases practically to exist. The author has also, independently of Berson, drawn up curves of a similar kind which have not as yet been published, and which he ventures to offer to the Physical Society, because they not only supplement Berson's results, but they seem also to partly explain why different observers have obtained such widely differing temperatures for the point of *nil* permeability‡.

In the axis of a magnetizing solenoid, and perpendicular to the magnetic meridian, was placed a nickel wire, 30 centim. in length and 0.0053 square centim. in section. The solenoid consisted of cotton-covered copper wire of  $\frac{1}{20}$  inch in diameter, wrapped in a single layer of 8.25 turns to the centimetre

\* Communicated by the Physical Society: read February 25, 1888.

† *Ann. de Phys. et de Chim.* vol. vii. (1886).

‡ That is *practically* nil. The experiments of Faraday seem to show that the permeability never *entirely* vanishes.



round a brass tube slit throughout its entire length, and having an internal diameter of 2·5 centim. Before being wrapped round the brass tube the wire was well coated with pipeclay moistened in water. Round 10 centim. of the central portion of the solenoid were wrapped 240 turns of another piece of the same wire, also coated with pipeclay, to serve as a secondary coil. The whole was then introduced into an air-chamber and the wet pipeclay allowed to dry, at first slowly at the temperature of the air, and afterwards more quickly at temperatures which were gradually raised to 400° C. The air-chamber consisted of two concentric copper cylinders 40 centim. in length, enclosing between them an annular space about ·5 centim. thick which was filled with fine sand, and was heated by a row of burners placed underneath. The temperature of the air-chamber was calculated from the alteration of electrical resistance of a coil of platinum wire, whose resistance had been very carefully determined at different temperatures up to 100° C., and expressed in terms of the temperature by a formula of the form

$$R_t = R_0(1 + at - bt^2),$$

where  $R_t$  and  $R_0$  are the resistances at  $t^\circ$  C. and  $0^\circ$  C. respectively, and  $a$  and  $b$  are constants. The platinum coil was wound double and was placed inside the slit brass tube close to the nickel wire, but insulated from it and the tube by asbestos; the length of the coil was 10 centim., and it occupied the central portion of the air-chamber. To the ends of the coil were hard-soldered stout copper terminal rods, which passed through a wooden cap closing one end of the air-chamber, and served to connect with a Wheatstone-bridge arrangement employed for determining the resistance of the platinum. The other end of the air-chamber was also closed with a wooden cap, through holes in which passed the ends of the wires of the magnetizing solenoid and the secondary coil. After the pipeclay coating had become thoroughly dry the resistances of the primary and secondary coils were tested, and found to be sensibly the same as before winding. The object of coating the cotton covering of the wires with pipeclay was to maintain the insulation, for at the temperatures reached in some of the experiments the cotton became charred. Whilst the coils lay undisturbed the pipeclay coating served the purpose for which it was intended very well; but when they were removed from the air-chamber on the conclusion of the experiments, both the charred cotton and the pipeclay easily came away from the wire, and so rendered the coils useless.

A pair of primary and secondary coils exactly similar to the  
*Phil. Mag.* S. 5. Vol. 25. No. 156. May 1888. 2 C

first, except that they had no pipeclay coating and no nickel core inside, was kept buried in a box filled with well-dried sawdust and placed about two feet away from the air-chamber. The two primary coils were connected up in series with each other, a battery, a key, a rheostat, and a tangent-galvanometer whose constant had been previously carefully determined. The secondary coils were also connected up in series with each other, with a rheostat, a Thomson's reflecting-galvanometer, and an earth-coil. The earth-coil consisted of seven turns of silk-covered copper wire laid side by side round the circumference of a wooden disk 30 centim. in diameter; the disk was supported with its plane horizontal, but could be turned round a horizontal axis in either direction through an angle of 180 degrees. The ends of the wires round the disk were soldered to amalgamated copper disks revolving in mercury-cups, which last served to connect the earth-coil with the rest of the apparatus in series with it. The two pairs of primary and secondary coils were so arranged that the currents induced on opening or closing the battery-circuit by means of the key exactly balanced each other before the introduction of the nickel into one of them. When therefore the nickel was introduced, the observed deflection produced by opening or closing the primary circuit was entirely due to the magnetic permeability of the nickel; and by comparing this deflection with that produced by turning the earth-coil suddenly through 180° the permeability could be determined in absolute measure, since the vertical component of the earth's magnetic force at the place was known. The earth-coil also served another purpose, namely to secure with the aid of the rheostat the constancy of the sensitiveness of the Thomson-galvanometer. As the temperature of the air-chamber was raised, the total resistance in both primary and secondary circuits increased; but by altering the rheostats in these circuits until the deflection of the tangent-galvanometer in the one case, and of the Thomson-galvanometer, when the earth-coil was suddenly turned, in the other, became the same as before the heating, constancy both in the magnetizing force and in the sensitiveness of the Thomson-galvanometer was secured.

The mode of proceeding was as follows:—By means of the set of burners underneath the air-chamber the temperature was raised to nearly the highest point which it was desirable to attain; the burners were then adjusted until the temperature was either constant or very slowly rising or falling; this could always be secured by waiting for a sufficient length of time. The rheostat in the secondary circuit was then adjusted

until the deflection of the Thomson-galvanometer, produced by suddenly turning the earth-coil, was exactly 80 divisions of the scale. The rheostat in the primary circuit was next adjusted until the required current had been reached, care being taken to begin with a small current, which was increased very gradually to the required amount. As it was the aim of the author to test only the *temporary* permeability\* of the nickel, the current in the primary circuit was opened and closed a great many times, until the current induced on closing the circuit became the same in magnitude as that induced on opening the circuit; and then the mean of the deflections produced by ten times closing the circuit was taken to measure the temporary induction. The air-chamber was now allowed to cool a little, and then, as before, after the temperature had been steadied by adjusting the burners, a fresh set of observations was made, and so on until the temperature of the room was reached. As soon as the observations with the lowest magnetizing force had been completed, fresh ones were made with a higher magnetizing force until a magnetizing force of 18·183 C.G.S. units had been reached.

The temporary permeability of the nickel was calculated from the following formulæ:—

$$M_f = \frac{4\pi n_1 C}{l}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$M_p = \frac{2NAVd}{DSn_2 M_f} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In formula (1)  $M_f$  is the magnetizing force,  $n_1$  is the number of turns in the primary coil,  $l$  the length of the coil, and  $C$  the current circulating round the coil.

In formula (2)  $M_p$  is the magnetic permeability (ratio of magnetic induction to magnetizing force),  $N$  the number of turns in the earth-coil,  $A$  the area of the earth-coil,  $D$  the deflection produced by suddenly turning the earth-coil through  $180^\circ$ ,  $V$  is the vertical component of the earth's magnetic force,  $d$  the induction-current due to the nickel,  $S$  the section of the nickel, and  $n_2$  the number of turns in the secondary coil. C.G.S. units were used throughout. Since the length of the nickel under the magnetizing force was about 350 times the diameter, the effect of the ends is quite negligible. The

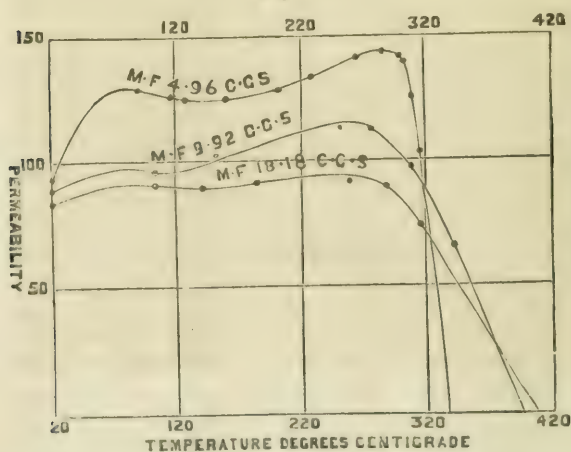
\* It is impossible to obtain *true* relations between change of temperature and change of *total* permeability by the "ballistic method," because the mere act of changing the temperature shakes out some of the sub-permanent magnetism which the metal may have acquired at previous temperatures.

results are given in the following table, which is supplemented by the curves in fig. 1. In fig. 2 are given Berson's curves for nickel, and in fig. 3 similar curves obtained by Ledebor\* for iron.

TABLE I.

Magnetizing force. } 4.959		6.612		9.918.		11.571.		18.183.	
Temperature, in degrees Centigrade, $t$ .	Permeability, $M_p$ .	$t$ .	$M_p$ .	$t$ .	$M_p$ .	$t$ .	$M_p$ .	$t$ .	$M_p$ .
16.5	93.4	19.5	88.8	20	88.6	19	87.6	18	82.0
84.5	127.9	55.0	97.1	54	93.4	47	91.1	60	84.4
117.6	125.7	93	101.0	74	93.4	84	89.8	79	87.6
128.0	125.0	127	97.6	83	94.1	133	95.2	102	90.8
160.0	126.4	160	107.0	102	95.5	178	97.4	143	89.6
200.0	127.9	186	108.1	143	102.9	191	101.5	174	91.6
227.0	130.9	227	120.7	157	103.7	275	105.6	186	92.6
261.0	141.2	279	126.3	204	106.3	286	104.0	254	92.4
282.0	144.1	301	111.4	247	111.8	295	100.9	293	90.2
297.0	142.6	349	60.7	262	112.1	330	63.0	316	72.2
304.0	139.7	.....	.....	275	112.9	346	54.2		
306.5	125.0	.....	.....	308	96.3				
317.0	102.9	.....	.....	340	65.1				

Fig. 1.



\* Both these and Berson's curves are taken from the 'Electrician,' vol. xx. No. 505.



Fig. 2.

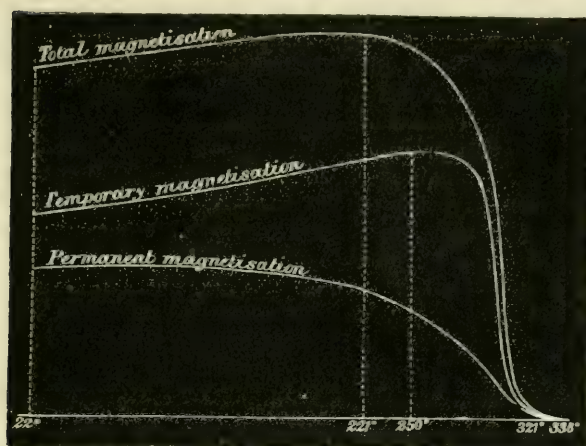
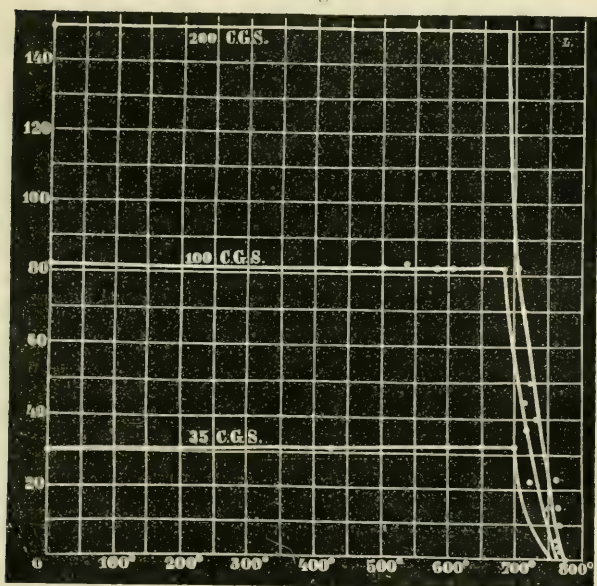


Fig. 3.



It seems from the above table and the curves in fig. 1 that the temperature of maximum permeability is lower the greater the magnetizing force. Thus, for the magnetizing forces 4.959, 9.918, and 18.183, we get the temperatures of maximum permeability as 287°, 248°, and 242° C. respectively.

The temperature at which the permeability practically vanishes seems, on the other hand, to be higher the greater the magnetizing force. As for the above-mentioned magnetizing forces, we have for this temperature the values  $333^{\circ}$ ,  $392^{\circ}$ , and  $412^{\circ}$  C. respectively. In this respect the behaviour of nickel resembles that of iron, as is evidenced by the curves in fig. 3; and it is probably partly for this reason that different observers have obtained different results for the temperature of *nil*-permeability. Another cause for the discrepancies in this respect between the observations of different experimenters is probably to be found in the want of purity of the metal; for it is evident that if iron be present as an impurity, the point of *nil*-permeability may be rendered very much higher\*. The observations made by the author were not in any case carried right up to the vanishing-point of the permeability, as the nickel used by him was, though nearly, not quite pure†; and it is not at all unlikely that, had this been done, the terminations of the curves would more nearly resemble those of Berson, shown in fig. 2, where it will be noticed that there is a point of inflexion before  $320^{\circ}$  C. The author does not believe that, even with perfectly pure nickel, the magnetic permeability would *entirely* vanish; but the following experiment will show that, with a thin layer of pure nickel, the permeability *practically* vanishes with great suddenness:—A brass wire 1 millim. in diameter was coated with a thin layer of nickel by electrolysis, and a piece of it, about 2 centim. in length, was suspended horizontally in a cradle of platinum between the pole-pieces of a powerful electromagnet in a direction perpendicular to the lines of magnetic force; the platinum cradle was supported by a platinum wire 12 inches long and  $\frac{1}{160}$  inch in diameter. The nickel-plated brass wire was heated by a burner to a temperature of visible red, and the electromagnet was then excited by five Grove-cells. Not the slightest effect of the intense field seemed to be experienced by the nickel, even after it had been cooling for several seconds. The magnetizing circuit was now broken, leaving only the residual magnetism in the pole-pieces, when, after a few seconds of further cooling, and without *the least preliminary warning*, the wire set axially with startling rapidity.

\* The temperature at which the permeability of iron vanishes seems, from Ledebour's curves, to lie between  $750^{\circ}$  and  $770^{\circ}$  C.

† The wire was procured from Messrs. Johnson and Matthey, who informed the author that they found it impossible to draw pure nickel wire. Mr. G. S. Johnson, the Demonstrator of Chemistry at King's College, London, has kindly furnished an analysis of the wire. It contains 97.5 per cent. of nickel and only 0.67 per cent. of iron.

The chief aim of the author in this research was to fix approximately the temperature at which the rate of loss of permeability begins to be greatest; and this appears to be  $300^{\circ}$  C., not only for the specimen of nickel used by him, but also for that used by Berson, and this, too, both for the temporary and total magnetization. The following table shows the rate at which the temporary permeability decreases with rise of temperature between  $300^{\circ}$  and  $320^{\circ}$  C.

TABLE II.

Temperature, in degrees C., $t$ .	Magnetizing force, $M_f$ .	Permeability, $M_p$ .	Differences between values of $M_p$ at $300^{\circ}$ and $320^{\circ}$ C. divided by 20, $\frac{\Delta M_p}{\Delta t}$ .	$\frac{\Delta M_p}{\Delta t} \times M_f$ .
300 320	} 4.959 {	141.0 96.6	} 2.22 {	11.1
300 320	} 11.571 {	95.5 73.8	} 1.08 {	12.5
300 320	} 18.183 {	84.8 69.1	} 0.79 {	14.3

From this table it is apparent that the rate of decrease of permeability is less the higher the magnetizing force; so much so indeed that whereas, with a magnetizing force of about 5 C.G.S. units, there is for each degree rise of temperature a decrease of 11 lines of force per square centimetre; with a force of 18 the loss of lines per degree is only increased to 14.

XLIX. *On Electromagnetic Waves, especially in relation to the Vorticity of the Impressed Forces; and the Forced Vibrations of Electromagnetic Systems.* By OLIVER HEAVISIDE.

[Continued from p. 156.]

### *Spherical Electromagnetic Waves.*

15. **L**EAVING the subject of plane waves, those next in order of simplicity are the spherical. Here, at the very beginning, the question presents itself whether there can be anything resembling condensational waves?

Sir W. Thomson (Baltimore Lectures, as reported by Forbes in 'Nature,' 1884) suggested that a conductor charged rapidly alternately + and - would cause condensational waves in the æther. But there is no other way of charging it than by

a current from somewhere else, so he suggested two conducting spheres to be connected with the poles of an alternating dynamo. The idea seems to be here that electricity would be forced out of one sphere and into the other to and fro with great rapidity, and that between the spheres there might be condensational waves.

But in this case, according to the Faraday law of induction, the result would be the setting up of alternating electromagnetic disturbances in the dielectric, exposing the bounding surfaces of the two spheres to rapidly alternating magnetizing and electrizing force, causing waves, approximately spherical at least, to be transmitted into the spheres, in the diffusion manner, greatly attenuating as they progressed inward.

Perhaps, however, there can be condensational waves if we admit that a certain quite hypothetical something called electricity is compressible, instead of being incompressible, as it must be if we in Maxwell's scheme make the unnecessary assumption that an electric current is the motion through space of the something. In fact, Prof. J. J. Thomson has calculated\* the speed of condensational waves supposed to arise by allowing the electric current to have convergence. But a careful examination of his equations will show that the condensational waves there investigated do not exist, *i. e.* the function determining them has the value zero†.

16. To construct a perfectly general spherical wave we may proceed thus. The characteristic equation of  $\mathbf{H}$ , the magnetic force, in a homogeneous medium free from impressed force is, by (2) and (3),

$$\nabla^2 \mathbf{H} = (4\pi\mu kp + \mu cp^2)\mathbf{H}. \quad . \quad . \quad . \quad (93)$$

Now, let  $\mathbf{r}$  be the vector distance from the origin, and  $Q$  any scalar function satisfying this equation. Let

$$\mathbf{H} = \text{curl}(\mathbf{r}Q). \quad . \quad . \quad . \quad . \quad (94)$$

Then this derived vector will satisfy (93), and have no convergence, and have no radial component, or will be arranged in spherical sheets. From it derive the other electromagnetic quantities. Change  $\mathbf{H}$  to  $\mathbf{E}$  to obtain spherical sheets of electric force.

This method leads to the spherical sheets depending upon any kind of spherical harmonic. They are, however, too general to be really useful except as mathematical exercises. For the examination of the manner of origin and propagation

\* B. A. Report on Electrical Theories.

† I ought to qualify this by adding that the investigation seems very obscure, so that although I cannot make the system work, yet others may.



of waves, zonal harmonics are more useful, besides leading to the solution of more practical problems. It is then not difficult to generalize results to suit any kind of spherical harmonic.

17. *The simplest Spherical Waves.*—Let the lines of  $\mathbf{H}$  be circles, centred upon the axis from which  $\theta$  is measured, and let  $r$  be the distance from the origin. We have no concern with  $\phi$  (longitude) as regards  $\mathbf{H}$ , so that the simple specification of its intensity  $H$  fully defines it. Under these circumstances the equation (93) becomes

$$\left. \begin{aligned} (rH)'' + \frac{\nu}{r^2}(\nu rH)'' &= (4\pi\mu_0 kp + \mu_0 cp^2)H, \\ &= q^2 H, \text{ say,} \end{aligned} \right\} \quad (95)$$

where the acute accent denotes differentiation to  $r$ , and the grave accent to  $\cos \theta$  or  $\mu$ , whilst  $\nu$  stands for  $\sin \theta$ . The inductivity will be now  $\mu_0$ , to avoid confusing with the  $\mu$  of zonal harmonics. Equation (95) also defines  $q$  in the three forms it can assume in a conductor, dielectric, and conducting dielectric.

Now try to make of  $r\mathbf{H}$  an undistorted spherical wave, *i. e.*  $H$  varying inversely as the distance, and travelling inward or outward at speed  $v$ . Let

$$rH = A f(r - vt), \quad \dots \dots \dots (96)$$

where  $A$  is independent of  $r$  and  $t$ . Of course we must have  $k=0$ , making  $q=p/v$ . Now (96) makes

$$v^2(rH)'' = rp^2 H; \quad \dots \dots \dots (97)$$

which, substituted in (95), gives

$$\nu(\nu H)'' = 0; \quad \dots \dots \dots (98)$$

therefore

$$A\nu = A_1\mu + B_1. \quad \dots \dots \dots (99)$$

From these we find the required solutions to be

$$H = E/\mu_0 v = \frac{A_1\mu + B_1}{r\nu} F_0'(r - vt), \quad \dots \dots \dots (100)$$

$$F = \mu_0 v \frac{A_1}{r^2} F_0(r - vt); \quad \dots \dots \dots (101)$$

where  $F_0$  is any function,  $A_1$  and  $B_1$  constants,  $E$  and  $F$  the two components of the electric force,  $F$  being the radial component out, and  $E$  the other component coinciding with a line of longitude, the positive direction being that of increasing  $\theta$ , or from the pole. Similarly, if the lines of  $\mathbf{E}$  be circular

382 Mr. O. Heaviside on *Electromagnetic Waves*, and the  
about the axis, we have the solutions

$$E = -\mu_0 v H_\theta = -\mu_0 v \frac{A_1 \mu + B_1}{r v} F_0'(r-vt), \quad (102)$$

$$H_r = \frac{A_1}{r^2} F_0(r-vt), \quad . \quad . \quad . \quad (103)$$

where  $H_r$  and  $H_\theta$  are the radial and tangential components of  $\mathbf{H}$ .

But both these systems involve infinite values at the axis. We must therefore exclude the axis somehow to make use of them. Here is one way. Describe a conical surface of any angle  $\theta_1$ , and outside it another of angle  $\theta_2$ , and let the dielectric lie between them. Make the tangential component of  $\mathbf{E}$  at the conical surfaces vanish, requiring infinite conductivity there, and we make  $F$  vanish in (101), and produce the solution

$$E = \mu_0 v H = \frac{B}{r v} f(r-vt), \quad . \quad . \quad . \quad (104)$$

exactly resembling plane waves as regards  $r v E$ . Here  $B$  is the same as  $\mu_0 v B_1$ , and  $f$  the same as  $F_0'$ , in equation (100)\*.

18. Now bring in zonal harmonics. Split equation (95) into the two

\* In order to render this arrangement (104) intelligible in terms of more everyday quantities, let the angles  $\theta_1$  and  $\theta_2$  be small, for simplicity of representation; then we have two infinitely conducting tubes of gradually increasing diameter enclosing between them a non-conducting dielectric. Now change the variables. Let  $V$  be the line-integral of  $\mathbf{E}$  across the dielectric, following the direction of the force; it is the potential-difference of the conductors. Let  $4\pi C$  be the line-integral of  $\mathbf{H}$  round the inner tube: it is the same for a given value of  $r$ , independent of  $\theta$ ;  $C$  is therefore what is commonly called the current in the conductor. We shall have

$$V = L v C, \quad C = S v V, \quad L S v^2 = 1;$$

where  $L$  is the inductance and  $S$  the permittance, per unit length of the circuit. The value of  $L$  is

$$L = 2\mu_0 \log [(\tan \frac{1}{2}\theta_2) \div (\tan \frac{1}{2}\theta_1)];$$

so that the circuit has uniform inductance and permittance. The value of  $C$  in terms of (104) is

$$C = \frac{B}{2\mu_0 v} f(r-vt).$$

When the tubes have constant radii  $a_1$  and  $a_2$ , the value of  $L$  reduces to the well known

$$L = 2\mu_0 \log (a_2/a_1),$$

of concentric cylinders. The wave may go either way, though only the positive wave is mentioned.

$$(rH)'' = \left\{ q^2 + \frac{m(m+1)}{r^2} \right\} rH, \quad . \quad . \quad . \quad (105)$$

$$\frac{\nu}{r^2} (\nu H)'' = - \frac{m(m+1)}{r^2} H. \quad . \quad . \quad . \quad (106)$$

The equation (106) has for solution

$$H = A \nu Q'_m,$$

where A is independent of  $\theta$ , and is to be found from (105).

The most practical way of getting the  $r$  functions is that followed by Professor Rowland in his paper\*, wherein he treats of the waves emitted when the state is sinusoidal with respect to the time. We shall come across the same waves in some problems.

Let

$$H = P_m \frac{\epsilon^{qr}}{r} \nu Q'_m. \quad . \quad . \quad . \quad (107)$$

Then the equation of  $P_m$  is, by insertion of (107) in (105),

$$P'' + 2qP' = \frac{m(m+1)}{r^2} P; \quad . \quad . \quad . \quad (108)$$

and the solution, for practical purposes with complete harmonics, is

$$P = 1 - \frac{m(m+1)}{2qr} + \frac{m(m^2-1^2)(m+2)}{2 \cdot 4q^2r^2} - \frac{m(m^2-1)(m^2-2^2)(m+3)}{2 \cdot 4 \cdot 6q^3r^3} + \dots \quad (109)$$

We shall find the first few useful, thus :—

$$\left. \begin{aligned} P_1 &= 1 - (qr)^{-1}, \\ P_2 &= 1 - 3(qr)^{-1} + 3(qr)^{-2}, \\ P_3 &= 1 - 6(qr)^{-1} + 15(qr)^{-2} - 15(qr)^{-3}. \end{aligned} \right\} \quad . \quad (110)$$

Now let  $U = \epsilon^{qr}P$ , so that U is the  $r$  function in Hr. If we change the sign of  $q$  in U, producing, say, W, it is the required second solution of (105). Thus

$$U_1 = \epsilon^{qr} \left( 1 - \frac{1}{qr} \right), \quad W_1 = \epsilon^{-qr} \left( 1 + \frac{1}{qr} \right), \quad . \quad . \quad (111)$$

in the very important case of  $Q_1$ , when  $m=1$ .

\* Phil. Mag. June 1884, "On the Propagation of an Arbitrary Electromagnetic Disturbance, Spherical Waves of Light, and the Dynamical Theory of Refraction." Prof. J. J. Thomson has also considered spherical waves in a dielectric in his paper "On Electrical Oscillations and Effects produced by the Motion of an Electrified Sphere," Proc. London Math. Soc. vol. xv., April 3, 1884.

The conjugate property of  $U$  and  $W$  is

$$UW' - U'W = -2q, \quad . \quad . \quad . \quad . \quad . \quad (112)$$

which is continually useful.

We have next to combine  $U$  and  $W$  so as to produce functions suitable for use inside spheres, right up to the centre, and finite there. Let

$$u = \frac{1}{2}(U + W), \quad w = \frac{1}{2}(U - W). \quad . \quad . \quad . \quad . \quad (113)$$

It will be found that when  $m$  is even,  $w/r$  is zero and  $u/r$  infinite at the origin; but that when  $m$  is odd, it is  $u/r$  that is zero at the origin and  $w$  infinite.

The conjugate property of  $u$  and  $w$  is

$$uw' - u'w = q, \quad . \quad . \quad . \quad . \quad . \quad (114)$$

corresponding to (112).

19. *Construction of the Differential Equations connected with a Spherical Sheet of Vorticity of Impressed Force.*—Now let there be two media—one extending from  $r=0$  to  $r=a$ , in which we must therefore use the  $u$  function or  $w$  function, according as  $m$  is odd or even, and an outer medium, or at least one in which  $q$  has a different form in general. Then, within the sphere of radius  $a$ , we have

$$H = Ar^{-1}u, \quad . \quad . \quad . \quad . \quad . \quad (115)$$

$$-k_1 E = Ar^{-1}u', \quad . \quad . \quad . \quad . \quad . \quad (116)$$

where  $k_1 = 4\pi k + cp$ , and we suppose  $m$  odd. It follows that

$$\frac{E}{H} = -\frac{1}{k_1} \frac{u'}{u}. \quad . \quad . \quad . \quad . \quad . \quad (117)$$

In the outer medium use  $W$ , if the medium extends to infinity, or both  $U$  and  $W$  if there be barriers or change of medium. First, let it be an infinitely extended medium. Then, in it,

$$H = Br^{-1}(u - w), \quad . \quad . \quad . \quad . \quad . \quad (118)$$

$$-k_2 E = Br^{-1}(u' - w'), \quad . \quad . \quad . \quad . \quad . \quad (119)$$

where  $k_2 = 4\pi k + cp$  in the outer medium. From these

$$\frac{E}{H} = -\frac{1}{k_2} \frac{u' - w'}{u - w}. \quad . \quad . \quad . \quad . \quad . \quad (120)$$

(117) and (120) show the forms of the resistance-operators on the two sides\*.

\* Some rather important considerations are presented here. On what principles should we settle which functions to use, internally and externally, seeing that these functions  $U$  and  $W$  are not quantities, but differential operators? First, as regards the space outside the surface of origin



Now, at the surface of separation,  $r=a$ ,  $H$  is continuous (unless we choose to make it a sheet of electric current, which we do not); so that the  $H$  in (117) and in (120) are the same. We only require a relation between the  $E$ 's to complete the differential equation.

Let there be vorticity of impressed force on the surface  $r=a$ , and nowhere else (the latter being already assumed). Then

$$\text{curl } \mathbf{e} = \text{curl } \mathbf{E} \quad . \quad . \quad . \quad . \quad . \quad (121)$$

is the surface-condition which follows; or, if  $f$  be the measure of the curl of  $\mathbf{e}$ ,

$$f = E_2 - E_1, \quad . \quad . \quad . \quad . \quad . \quad (122)$$

$E_2$  meaning the outer and  $E_1$  the inner  $E$ . Therefore

$$f = H_a \left( \frac{E_2}{H_2} - \frac{E_1}{H_1} \right), \quad . \quad . \quad . \quad . \quad . \quad (123)$$

$H_a$  denoting the surface  $H$ . So, by (117) and (120), used in (123),

$$f = \left( \frac{1}{k_1} \frac{u_1'}{u_1} - \frac{1}{k_2} \frac{u_2' - w_2'}{u_2 - w_2} \right) H_a, \quad (r=a), \quad . \quad . \quad (124)$$

the required differential equation. Observe that  $u_1$  only differs from  $u_2$  and  $w_1$  from  $w_2$  in the different values of  $q$  inside and outside (when different), and that  $r=a$  in all.

of disturbances. The operator  $\epsilon^{qr}$  turns  $f(t)$  into  $f(t+r/v)$ , and can therefore only be possible with a negative wave, coming to the origin. But there cannot be such a wave without a barrier or change of medium to produce it. Hence the operator  $\epsilon^{-qr}$  alone can be involved in the external solution when the medium is unbounded, and we must use  $W$ . Next, go inside the sphere  $r=a$ . It is clear that both  $U$  and  $W$  are now needed, because disturbances come to any point from the further as well as from the nearer side of the surface, thus coming from and going to the centre. Two questions remain: Why take  $U$  and  $W$  in equal ratio?; and why their sum or their difference, according as  $m$  is odd or even? The first is answered by stating the facts that, although it is convenient to assume the origin to be a place of reflexion, yet it is really only a place where disturbances cross, and that the  $H$  produced at any point of the surface is (initially) equal on both sides of it. The second question is answered by stating the property of the  $Q'_m$  function, that it is an even function of  $\mu$  when  $m$  is odd, and conversely; so that when  $m$  is odd the  $H$  disturbances arriving at any point on a diameter from its two ends are of the same sign, requiring  $U+W$ ; and when  $m$  is even, of opposite signs, requiring  $U-W$ .

Similar reasoning applies to the operators concerned in other than spherical waves. Cases of simple diffusion are brought under the same rules by generalizing the problem so as to produce wave-propagation with finite speed. On the other hand, when there are barriers, or changes of media, there is no difficulty, because the boundary conditions tell us in what ratio  $U$  and  $W$  must be taken.

Equation (124) applies to any odd  $m$ . When  $m$  is even, exchange  $u$  and  $w$ , also  $u'$  and  $w'$ . In the  $m$ th system we may write

$$f_m = \phi_m H_a, \quad . \quad . \quad . \quad . \quad . \quad (125)$$

the form of  $\phi$  being given in (124). The vorticity of the impressed force is of course restricted to be of the proper kind to suit the  $m$ th zonal harmonic. Thus, any distribution of vorticity whose lines are the lines of latitude on the spherical surface may be expanded in the form

$$\sum f_m \nu Q_m, \quad . \quad . \quad . \quad . \quad . \quad (126)$$

and it is the  $m$ th of these distributions which is involved in the preceding.

20. Both media being supposed to be identical,  $\phi$  reduces to

$$\phi = \frac{1}{k_1} \frac{q}{u_a(u_a - w_a)}, \quad . \quad . \quad . \quad . \quad . \quad (127)$$

by using (114) in (124). This is with  $m$  odd; if even, we shall get

$$\phi = \frac{1}{k_1} \frac{q}{w_a(u_a - w_a)}. \quad . \quad . \quad . \quad . \quad . \quad (128)$$

In a non-dielectric conductor,  $k_1 = 4\pi k$ , and  $q^2 = 4\pi\mu kp$ ; so that, keeping to  $m$  odd,

$$\phi = \left( \frac{\mu p}{4\pi k} \right)^{\frac{1}{2}} \frac{1}{u_a(u_a - w_a)}. \quad . \quad . \quad . \quad . \quad . \quad (129)$$

In a non-conducting dielectric,  $k_1 = cp$ , and  $q = p/\epsilon$ ; so

$$\phi = \frac{\mu_0 p}{u_a(u_a - w_a)}. \quad . \quad . \quad . \quad . \quad . \quad (130)$$

In this case the complete differential equation is

$$\Pi_a = \sum \frac{\nu(Q_m)}{\mu_0 v} u_a(u_a - w_a) f_m, \quad . \quad . \quad . \quad . \quad (131)$$

when there is any distribution of impressed force in space whose vorticity is represented by (126).

Outside the sphere, consequently

$$(\text{out}) \quad \left\{ \begin{array}{l} \Pi = \sum \frac{\nu(Q_m)}{\mu_0 r} \frac{a}{r} u_a(u - w) f_m, \quad . \quad . \quad . \quad (132) \\ -cp\Pi = \sum \frac{\nu(Q_m)}{\mu_0 r} \frac{a}{r} u (u' - w') f_m, \quad . \quad . \quad . \quad (133) \end{array} \right.$$

understanding that when no letter is affixed to  $u$  or  $w$ , the value at distance  $r$  is meant. We see at once that  $u_a = 0$  makes the external field vanish, *i. e.* the field of the particular

$f$  concerned. This happens, when  $f$  is a sinusoidal function of the time, at definite frequencies. Also, inside the sphere,

$$(\text{in}) \quad \begin{cases} H = \sum \frac{\nu Q_m}{\mu_0 v} \frac{a}{r} u(u_a - w_a) f_m, & . \quad . \quad (134) \\ -cpE = \sum \frac{\nu Q_m}{\mu_0 v} \frac{a}{r} u'(u_a - w_a) f_m. & . \quad . \quad (135) \end{cases}$$

As for the radial component  $F$ , it is not often wanted. It is got thus from  $H$  :—

$$-cpF = \frac{1}{r} (\nu H)', \quad . \quad . \quad . \quad . \quad (136)$$

where for  $cp$  write  $4\pi k + cp$  in the general case. Thus, the internal  $F$  corresponding to (135) is

$$(\text{in}) \quad cpF = \sum \frac{m(m+1)}{\mu_0 v} \frac{a}{r^2} u(u_a - w_a) f_m Q_m. \quad (137)$$

21. *Practical Problem. Uniform Impressed Force in the Sphere.*—If there be a uniform field of impressed force in the sphere, parallel to the axis, of intensity  $f_1$ , its vorticity is represented by  $f_1 \sin \theta$  on the surface of the sphere. It is therefore the case  $m=1$  in the above. Let this impressed force be suddenly started. Find the effect produced. We have, by (132) \*,

$$(\text{out}) \quad H = u_a(u - w) \frac{f_1 \nu a}{\mu_0 v r}; \quad . \quad . \quad . \quad (138)$$

\* It will be observed that the operator connecting  $f_1$  and  $H$  is of such a nature that the process of expansion of  $H$  in a series of normal functions fails. I have examined several cases of this kind. The invariable rule seems to be that when there is a surface of vorticity of  $\mathbf{e}$ , leading to an equation of the form  $f = \phi \Pi$ , and there is a change of medium somewhere, or else barriers, causing reflected waves, the form of  $\phi$  is such that we can, when  $f$  is constant, starting at  $t=0$ , solve thus

$$H = \frac{f}{\phi_0} + \sum \frac{f \epsilon p^t}{p(d\phi/dp)},$$

extending over all the (algebraical)  $p$  roots of  $\phi=0$ , which is the determinantal equation. But should there be no change of medium, the conjugate property of the functions concerned comes into play. It causes a great simplification in the form of  $\phi$ , and makes the last method fail completely, all traces of the roots having disappeared. But if we pass continuously from one case to the other, then the last formula becomes a definite integral. On the other hand, we can immediately integrate  $f = \phi H$  in its simplified form, and obtain an interpretable equivalent for the definite integral, which latter is more ornamental than useful. In the simplified form,  $\phi$  may be either rational or irrational. The integration of the irrational forms will be given in some later problems.

or, in full, referring to the forms of  $u$  and  $w$ , equations (110) to (113),

$$H = \frac{va}{2\mu_0 vr} \left\{ \epsilon^{-q(r-a)} \left( 1 - \frac{1}{qa} \right) \left( 1 + \frac{1}{qr} \right) + \epsilon^{-q(r+a)} \left( 1 + \frac{1}{qa} \right) \left( 1 + \frac{1}{qr} \right) \right\} f_1. \quad (139)$$

Effect the integrations indicated by the inverse powers of  $q$  or  $p/v$ ; thus

$$\frac{f_1}{q^n} = f_1 \frac{(vt)^n}{\frac{n}{v}}, \quad . \quad . \quad . \quad . \quad . \quad (140)$$

if  $f_1$  be zero before and constant after  $t=0$ . As for the exponentials, use Taylor's theorem, as only differentiations are involved. We get, after the process (140) has been applied to (139), and then Taylor's theorem carried out,

$$H = \frac{f_1 va}{2\mu_0 vr} \left\{ \left( 1 - \frac{vt_1}{a} + \frac{vt_1}{r} - \frac{v^2 t_1^2}{2ar} \right) + \left( 1 + \frac{vt_2}{a} + \frac{vt_2}{r} + \frac{v^2 t_2^2}{2ar} \right) \right\}, \quad (141)$$

where

$$vt_1 = vt - r + a,$$

$$vt_2 = vt - r - a.$$

It is particularly to be noticed that the  $t_1$  part of (141) only comes into operation when  $t_1$  reaches zero, and similarly as regards the  $t_2$  part. Thus, the first part expresses the primary wave out from the surface; the second, arriving at any point  $2a/r$  later than the first, is the reflected wave from the centre, arising from the primary wave inward from the surface.

The primary wave outward may be written

$$H = \left( \frac{f_1 v}{2\mu_0 v} \right) \frac{1}{2} \left( 1 + \frac{a^2 - v^2 t^2}{r^2} \right), \quad . \quad . \quad . \quad (142)$$

where  $vt > (r-a)$ , and the second wave by its exact negative, with  $vt > (r+a)$ . Now, by comparing (132) with (134), we see that the internal solution is got from the external by exchanging  $a$  and  $r$  in the  $\{\}$ 's in (139) and (141), including also in  $t_1$  and  $t_2$ . The result is that (142) represents the internal  $H$  in the primary inward wave,  $vt$  having to be  $> (a-r)$ ; whilst its negative represents the reflected wave, provided  $vt > (a+r)$ .

The whole may be summed up thus. First,  $vt$  is  $< a$ . Then (142) represents  $H$  everywhere between  $r=a+vt$  and  $r=a-vt$ . But when  $vt$  is  $> a$ ,  $H$  is given by the same formula between the limits  $r=vt-a$  and  $vt+a$ . In both cases  $H$  is zero outside the limits named.



The reflected wave, superimposed on the primary, annuls the H disturbance, which is therefore, after the reflexion, confined to a spherical shell of depth  $2a$  containing the uncanceled part of the primary wave outward.

The amplitude of H at the fronts of the two primary waves, in and out, before the former reaches the centre, is

$$(f_1va) \div (2\mu_0vr).$$

After the inward wave has reached the centre, however, the amplitude of H on the front of the reflected wave is the negative of that of the primary wave at the same distance, which is itself negative.

The process of reflexion is a very remarkable one, and difficult to fully understand. At the moment  $t=a/v$  that the disturbance reaches the centre, we have  $H=(f_1v) \div (4\mu_0v)$ , constant, all the way from  $r=0$  to  $2a$ , which is just half the initial value of H on leaving the surface of the sphere. But just before reaching the centre, H runs up infinitely for an infinitely short time, infinitely near the centre; and just after the centre is reached we have  $H=-\infty$  infinitely near the centre, where the H disturbance is always zero, except in this singular case when it is seemingly finite for an infinitely short time, though, of course,  $v$  is indeterminate.

With respect to this running-up of the value of H in the inward primary wave, it is to be observed that whilst H is increasing so fast at and near its front, it is falling elsewhere, viz. between near the front and the surface of the sphere; so that just before the centre is reached H has only half the initial value, except close to the centre, where it is enormously great.

After reflexion has commenced, the H disturbance is negative in the hinder part of the shell of depth  $2a$  which goes out to infinity, positive of course still in the forward part. At a great distance these portions become of equal depth  $a$ ; at its front  $H=(f_1va)(2\mu_0vr)^{-1}$ , at its back  $H=-$  ditto; using of course a different value of  $r$ .

22. As regards the electric field, we have, by (133),

$$(\text{out}) \quad E = -\frac{1}{cp} \frac{v}{\mu_0v} \frac{a}{r} u_a(u'-w')f_1; \quad \dots \quad (143)$$

which, expanded, is

$$E = \frac{va}{2r} \left\{ \epsilon^{q(a-r)} \left(1 - \frac{1}{qa}\right) \left(1 + \frac{1}{qr} + \frac{1}{q^2r^2}\right) + \epsilon^{-q(a+r)} \left(1 + \frac{1}{qa}\right) \left(1 + \frac{1}{qr} + \frac{1}{q^2r^2}\right) \right\} f_1; \quad (144)$$

390 Mr. O. Heaviside on *Electromagnetic Waves*, and the comparing which with (139), we see that

$$E = \mu r H + \frac{va}{2r} \left\{ \epsilon^{-q(r-a)} \frac{1}{q^2 r^2} \left( 1 - \frac{1}{qa} \right) + \epsilon^{-q(r+a)} \frac{1}{q^2 r^2} \left( 1 + \frac{1}{qa} \right) \right\} f_1. \quad (145)$$

We have, therefore, only to develop the second part, which is not in the same phase with  $H$ . It is, in the same manner as before,

$$\frac{f_1 v a}{2r} \left( \frac{r^2 t_1^2}{2r^2} - \frac{r^3 t_1^3}{6r^2 a} \right) + \frac{f_1 v a}{2r} \left( \frac{r^2 t_2^2}{2r^2} + \frac{r^3 t_2^3}{6r^2 a} \right), \quad \dots \quad (146)$$

only operating when  $vt_1 = vt - r + a$ , and  $vt_2 = vt - r - a$  are positive. Or,

$$\frac{f_1 v a}{4r^3} \left\{ \left( \frac{2a^2}{3} + a(vt-r) - \frac{(vt-r)^3}{3a} \right)_1 + \left( \frac{2a^2}{3} - a(vt-r) + \frac{(vt-r)^3}{3a} \right)_2 \right\}, \quad (146 a)$$

1 and 2 referring to the two waves. So, when  $vt > (r+a)$ , and the two are coincident, we have the sum

$$E = \frac{f_1 v a^3}{3r^3}, \quad \dots \quad (147)$$

which is the tangential component of the steady electric field left behind.

The radial component  $F$  is, by (137),

$$(\text{out}) \quad F = \frac{\cos \theta}{r} \left\{ \epsilon^{-q(r-a)} \left( \frac{1}{qr} + \frac{1}{q^2 r^2} - \frac{1}{q^2 r a} - \frac{1}{q^3 r^2 a} \right) + \dots \right\} f_1, \quad (148)$$

where the unwritten term  $\dots$  may be obtained from the preceding by changing the sign of  $a$ . Or,

$$F = \frac{f_1 a \cos \theta}{r^2} \left\{ \left( vt_1 + \frac{r^2 t_1^2}{2r} - \frac{r^2 t_1^2}{2a} - \frac{r^3 t_1^3}{6ra} \right) + \dots \right\}, \quad (149)$$

where  $vt_1 = vt + a - r$ . Or,

$$F = \frac{f_1 a \cos \theta}{r^2} \left\{ \frac{a^2}{3r} + \frac{a}{2} + \frac{a}{2r} (vt-r) - \frac{1}{2a} (vt-r)^2 - \frac{1}{6ra} (vt-r)^3 + \dots \right\}; \quad (150)$$

so that, when both waves coincide, we have their sum,

$$F = \frac{2f_1 a^3 \cos \theta}{3r^3}, \quad \dots \quad (151)$$

which is the radial component of the steady field left behind by the part of the primary wave whose magnetic field is wholly cancelled.

To verify, the uniform field of impressed force of intensity  $f_1$ , by elementary principles, produces the external electric potential

$$\Omega = f_1 \cos \theta \frac{a^3}{3r^2},$$

whose derivatives, radial and tangential, taken negatively, are (151) and (147). The corresponding internal potential is

$$\Omega = \frac{1}{3} f_1 r \cos \theta.$$

But its slope does not give the force  $\mathbf{E}$  left behind within the sphere, because this  $\mathbf{E}$  is the force of the flux. Any other distribution of impressed force, with the same vorticity, will lead to the same  $\mathbf{E}$ . Our equation (135) and its companion for  $\mathbf{F}$ , derived from (134) by using (136), lead to the steady field (residual)

$$\mathbf{E} = -\frac{2}{3} f_1 \sin \theta, \quad \mathbf{F} = \frac{2}{3} f_1' \cos \theta, \quad . \quad . \quad . \quad (152)$$

the components of the true force of the flux. Add  $\mathbf{e}$  to the slope of  $\Omega$  to produce  $\mathbf{E}^*$ .

$\mathbf{F}$  is always zero at the front of the primary wave outward, and  $\mathbf{E} = \mu_0 v \mathbf{H}$ . At the front of the primary wave inward  $\mathbf{F}$  is also zero, and  $\mathbf{E} = -\mu_0 v \mathbf{H}$ . After reflexion,  $\mathbf{F}$  at the front of the reflected wave is still zero, but now  $\mathbf{E} = \mu_0 v \mathbf{H}$ .

The electric energy  $U_1$  set up is the volume-integral of the scalar product  $\frac{1}{2} \mathbf{eD}$ . That is,

$$U_1 = \frac{1}{2} f_1 \times \frac{2}{3} \frac{c f_1}{4\pi} \times \frac{4\pi a^3}{3} = \frac{c a^3 f_1^2}{9} . \quad . \quad . \quad (153)$$

But the total work done by  $\mathbf{e}$  is  $2U_1$ , by the general law that the whole work done by impressed forces suddenly started exceeds the amount representing the waste by Joule heating at the final rate (when there is any), supposed to start at once, by twice the excess of the electric over the magnetic energy of the steady field set up. It is clear, then, that when the travelling shell has gone a good way out, and it has become nearly equivalent to a plane wave, its electric and magnetic energies are nearly equal, and each nearly  $\frac{1}{2} U_1$

\* Sometimes the flux is apparently wrongly directed. For example, a uniform field of impressed force from left to right in all space except a spherical portion produces a flux from right to left in that portion. This is made intelligible by the above. Let the impressed force act in the space between  $r=a$  and  $r=b$ ,  $a$  being small and  $b$  great. In the inner sphere the first effects are those due to the  $r=a$  vorticity, and the flux left behind is against the force. But after a time comes the wave from the  $r=b$  vorticity, which sets matters right. The same applies in the case of conductors, when, in fact, a long time might have to elapse before the second and real permanent state conquered the first one.

in value. I did not, however, anticipate that the magnetic energy in the travelling shell would turn out to be constant, viz.  $\frac{1}{2}U_1$  during the whole journey, from  $t=a/v$  to  $t=\infty$ , so that it is the electric energy in the shell which gradually decreases to  $\frac{1}{2}U_1$ . Integrate the square of  $H$  according to (142) to verify.

23. The most convenient way of reckoning the work done, and also the most appropriate in this class of problems, is by the integral of the scalar product of the curl of the impressed force and the magnetic force. Thus, in our problem

$$2U_1 = \int dt \Sigma \mathbf{e} \Gamma = \int dt \Sigma \mathbf{H} \text{ curl } \mathbf{e} / 4\pi \\ = \iint \frac{f_1 v dS}{4\pi} \int H_a dt, \quad . \quad . \quad . \quad . \quad (154)$$

where  $dS$  is an element of the surface  $r=a$ . So we have to calculate the time-integral of the magnetic force at the place of vorticity of  $\mathbf{e}$ , the limits being 0 and  $2a/v$ . This can be easily done without solving the full problem, not only in the case of  $m=1$ , but  $m$  = any integer. The result is, if  $U_m$  be the electric energy of the steady field due to  $f_m$ ,

$$\int H_a dt = \frac{caf_m v Q_m}{2m+1}, \quad . \quad . \quad . \quad . \quad (155)$$

and, therefore, by surface-integration according to (154),

$$2U_m = a^3 c f_m^2 \frac{m(m+1)}{2m+1}. \quad . \quad . \quad . \quad . \quad (156)$$

$\frac{1}{2}U_m$  is the magnetic energy in the  $m$ th travelling shell. I have entered into detail in the case of  $m=1$ , because of its relative importance, and to avoid repetition. In every case the magnetic field of the primary wave outward is cancelled by that of the reflexion of the primary wave inward, producing a travelling shell of depth  $2a$ , within which is the final steady field. There are, however, some differences in other respects, according as  $m$  is even or odd.

Thus, in the case  $m=2$ , we have, by (110) to (113),

$$\frac{1}{2}(U_a - W_a)W = \frac{1}{2} \left\{ \epsilon^{q(a-r)} \left( 1 - \frac{3}{qa} + \frac{3}{q^2 a^2} \right) \right. \\ \left. - \epsilon^{-q(r+a)} \left( 1 + \frac{3}{qa} + \frac{3}{q^2 a^2} \right) \right\} \times \left( 1 + \frac{3}{qr} + \frac{3}{q^2 r^2} \right). \quad (157)$$

Making this operate upon  $f_2$ , zero before and constant after  $t=0$ , we obtain, by (132), (140), and Taylor's theorem,

$$(\text{out}) \ H = \frac{f_2 a v Q_2}{2\mu_0 v r} \left\{ \frac{1}{4} + \frac{3}{8} \left( \frac{a^2}{r^2} + \frac{r^2}{a^2} \right) - r^2 t^2 \frac{3}{4} \left( \frac{1}{r^2} + \frac{1}{a^2} \right) + \frac{3}{8} \frac{r^4 t^4}{a^2 r^2} - \dots \right\} \quad (158)$$



In the wave represented,  $vt > (r-a)$ , it being the primary wave out. The unrepresented part, to be obtained by changing the sign of  $a$  within the  $\{\}$ , is the reflected wave, in which  $vt > (r+a)$ .

To obtain the internal H exchange  $a$  and  $r$  within the  $\{\}$  in (158). The result is that

$$H = \frac{f_2 a v Q_2'}{2 \mu_0 v r} \left\{ -\frac{1}{2} + \frac{3}{8 a^2 r^2} (v^2 t^2 - a^2 - r^2)^2 \right\} . \quad (159)$$

expresses the H solution always, provided that when  $vt < a$  the limits for  $r$  are  $a-vt$  and  $a+vt$ ; but when  $vt > a$ , they are  $vt-a$  and  $vt+a$ .

At the surface of the sphere,

$$H_a = \frac{f_2 v Q_2'}{2 \mu_0 v} \left\{ 1 - \frac{3}{2} \left( \frac{vt}{a} \right)^2 + \frac{3}{8} \left( \frac{vt}{a} \right)^4 \right\}, \quad . \quad (160)$$

from  $t=0$  to  $2a/v$ . It vanishes twice, instead of only once, intermediately, finishing at the *same* value that it commenced at, instead of at the opposite, as in the  $m=1$  case.

The radial component F of E is always zero at the front of either of the primary waves or of the reflected wave, and  $E = \pm \mu_0 v H$ , according as the wave is going out or in. In the travelling shell H changes sign  $m$  times, thus making  $m+1$  smaller shells of oppositely directed magnetic force. At its outer boundary

$$E = \mu_0 v H = \frac{1}{2} f_m v Q_m'(a/r), \quad . \quad . \quad . \quad (161)$$

and at the inner boundary the same formula holds, with  $\pm$  prefixed according as  $m$  is even or odd.

In case  $m=3$ , the magnetic force at the spherical surface is

$$H_a = \frac{f_3 v Q_3'}{2 \mu_0 v} \left\{ 1 - \frac{3 v^2 t^2}{a^2} + \frac{15}{8} \frac{v^4 t^4}{a^4} - \frac{5}{16} \frac{v^6 t^6}{a^6} \right\} . \quad (162)$$

from  $t=0$  to  $2a/v$ , after which, zero.

24. *Spherical Sheet of Radial Impressed Force.*—If the surface  $r=a$  be a sheet of radial impressed force, it is clear that the vorticity is wholly on the surface. Let the intensity be independent of  $\phi$ , so that

$$e = \Sigma e_m Q_m. \quad . \quad . \quad . \quad . \quad . \quad (163)$$

The steady potential produced is

$$(\text{in}) \quad V_1 = -\Sigma e_m Q_m \frac{m+1}{2m+1} \left( \frac{r}{a} \right)^m . \quad (164)$$

$$(\text{out}) \quad V_2 = +\Sigma e_m Q_m \frac{m}{2m+1} \left( \frac{a}{r} \right)^{m+1}, \quad (165)$$

because, at  $r=a$ , these make

$$V_2 - V_1 = e, \quad \text{and} \quad \frac{dV_1}{dr} = \frac{dV_2}{dr}; \quad . \quad . \quad (166)$$

i. e. potential-difference  $e$  and continuity of displacement. The normal component of displacement is

$$-\frac{c}{4\pi} \frac{dV_1}{dr} = \frac{c}{4\pi a} \Sigma e_m Q_m \frac{m(m+1)}{2m+1}; \quad . \quad . \quad . \quad (167)$$

therefore, integrating over the sphere, the total work done by  $e$  is

$$2U = \Sigma e a e_m^2 \frac{m(m+1)}{(2m+1)^2}, \quad . \quad . \quad . \quad (168)$$

which agrees with the estimate (156), because

$$f = -\frac{de}{a d\theta} = \frac{\nu}{a} \frac{de}{d\mu}, \quad . \quad . \quad . \quad (169)$$

finds the vorticity,  $f$ , from the radial impressed force  $e$ ; or, taking

$$e = e_m Q_m$$

$$\therefore \frac{e_m \nu Q_m'}{a} = \text{vorticity},$$

so that the old  $f_m = e_m/a$ .

25. *Single Circular Vortex Line*.—There are some advantages connected with transferring the impressed force to the surface of the sphere, as it makes the force of the flux and the force of the field identical both outside and inside. At the boundary  $F$  is continuous,  $E$  discontinuous.

Let the impressed force be a simple circular shell of radius  $a$ , and strength  $e$ . Let it be the equatorial plane, so that the equator is the one line of vorticity. Substitute for this shell a spherical shell of strength  $\frac{1}{2}e$  on the positive hemisphere,  $-\frac{1}{2}e$  on the negative, the impressed force acting radially. Expand this distribution in zonal harmonics. The result is

$$\Sigma e_m Q_m = \frac{e}{2} \left\{ \frac{3}{2} Q_1 - \frac{7}{2 \cdot 4} Q_3 + \frac{11 \cdot 3}{2 \cdot 4 \cdot 6} Q_5 - \frac{15 \cdot 1 \cdot 3 \cdot 5}{2 \cdot 4 \cdot 6 \cdot 8} Q_7 + \dots \right\}, \quad . \quad . \quad (170)$$

so that we are only concerned with the odd  $m$ 's. This equation settling the value of  $e_m$ , the vorticity is

$$\Sigma \frac{e_m \nu Q_m'}{a} = \Sigma f_m \nu Q_m'. \quad . \quad . \quad . \quad (171)$$

We know therefore by the preceding, the complete solution due to sudden starting of the single vortex line. That is, we know the individual waves in detail produced by  $e_1, e_3$ , &c. The resultant travelling disturbance is therefore confined between two spherical surfaces of radii  $vt-a$  and  $vt+a$ , after the centre has been reached, or of radii  $a-vt$  and  $a+vt$  before the centre is reached. But it cannot occupy the whole of either of the regions mentioned.

The actual shape of the boundaries, however, may be easily found. It is sufficient to consider a plane section through the axis of the sphere. Let A and B be the points on this plane cut by the vortex line. Describe circles of radius  $vt$  with A and B as centres. If  $vt < a$ , the circles do not intersect; the disturbance is therefore wholly within them. But when  $vt$  is  $> a$ , the intersecting part contains no **H**, and only the **E** of the steady field due to the vortex line, which we know by § 24.

That within the part common to both circles there is no **H** we may prove thus. The vortex line in question may be imagined to be a line of latitude on any spherical surface passing through A and B, and centred upon the axis. Let  $a_1$  be the radius of any sphere of this kind. Then, at a time making  $vt > a$ , the disturbance must lie between the surfaces of spheres of radii  $vt-a_1$  and  $vt+a_1$ , whose centre is that of the sphere  $a_1$ . Now this excludes a portion of the space between the  $vt-a$  and  $vt+a$  circles, referring to the plane section; and by varying the radius  $a_1$  we can find the whole space excluded. Thus, find the locus of intersections of circles of radius

$$vt - (a^2 + z^2)^{\frac{1}{2}},$$

with centre at distance  $z$  from the origin, upon the axis. The equation of the circle is

$$(x-z)^2 + y^2 = \{vt - (a^2 + z^2)^{\frac{1}{2}}\}^2,$$

or

$$x^2 + y^2 - 2xz = v^2t^2 + a^2 - 2vt(a^2 + z^2)^{\frac{1}{2}}. \quad (172)$$

Differentiate with respect to  $z$ , giving

$$z(v^2t^2 - a^2)^{\frac{1}{2}} = av, \quad (173)$$

and eliminate  $z$  between (173) and (172). After reductions, the result is

$$x^2 + (y \pm a)^2 = v^2t^2, \quad (174)$$

indicating two circles, both of radius  $vt$ , whose centres are at A and B. Within the common space, therefore, the steady electric field has been established.

If this case be taken literally, then, since it involves an infinite concentration in a geometrical line of a finite amount of vorticity of  $\mathbf{e}$ , the result for the steady field is infinite close up to that line, and the energy is infinite. But imagine, instead, the vorticity to be spread over a zone at the equator of the sphere  $r=a$ , half on each side of it, and its surface-density to be  $f_1\nu$ , where  $f_1$  is finite. Consider the effect produced at a point in the equatorial plane. From time  $t=0$  to  $t_1=r-a$  (if the point be external) there is no disturbance. But from time  $t_1$  to  $t_2=b/r$ , where  $b$  is the distance from the point to the edges of the zone, the disturbance must be identically the same as if the harmonic distribution  $f_1\nu$  were complete, viz. by (142),

$$H = \left( \frac{f_1\nu}{2\mu_0 r} \right)^{\frac{1}{2}} \left( 1 + \frac{a^2 - r^2 t^2}{r^2} \right). \quad \dots \quad (175)$$

After this moment  $t_2$ , the formula of course fails. Now narrow the band to width  $ad\theta$  at the equator and simultaneously increase  $f_1$ , so as to make  $f_1 ad\theta = e$ , the strength of the shell of impressed force when there is but one. The formula (175) will now be true only for a very short time, and in the limit it will be true only momentarily, at the front of the wave, viz.

$$f_1 a / 2\mu_0 r = H = e / 2\mu_0 r d\theta, \quad \dots \quad (176)$$

going up infinitely as  $d\theta$  is reduced. To avoid infinities in the electric and magnetic forces we must seemingly keep either to finite volume or finite surface-density of vorticity of  $\mathbf{e}$ , just as in electrostatics with respect to electrification.

Instead of a simple shell of impressed electric force, it may be one of magnetic force, with similar results. As a verification calculate the displacement through circle  $\nu$  on the sphere  $r=a$  due to a vortex circle at  $\nu_1$  on the same surface, the latter being of unit strength. It is

$$\Sigma \frac{av}{2} \frac{ee_m \nu Q_m}{2m+1}, \quad \dots \quad (177)$$

due to  $\Sigma e_m Q_m$ , through the circle  $\nu$ . Take then

$$e_m = \frac{(2m+1)\nu_1^2 Q_{1m}}{2m(m+1)}, \quad \dots \quad (178)$$

which represents  $e_m$  due to vortex line of unit strength at  $\nu_1$ . Use this in the preceding equation (177) and we obtain

$$D = \Sigma \frac{ea}{4} \frac{\nu^2 Q_m \nu_1^2 Q_{1m}}{m(m+1)} \quad \dots \quad (179)$$



as the displacement through  $\nu$  due to unit vortex line at  $\nu_1$ . Applying this result to a circular electric current,  $\mathbf{B} = \mu_0 \mathbf{H}$  takes the place of  $\mathbf{D} = (c/4\pi)\mathbf{E}$ , as the flux concerned, whilst if  $h$  be the strength of the shell of impressed magnetic force,  $h/4\pi$  is the equivalent bounding electric current. The induction through the circle  $\nu$  due to unit electric current in the circle  $\nu_1$  is therefore obtainable from (179) by turning  $c$  to  $\mu_0$  and multiplying by  $(4\pi)^2$ . The result agrees with Maxwell's formula for the coefficient of mutual induction of two circles (vol. ii. art. 697).

It must be noted that in the magnetic-shell application there must be no conductivity, if the wave-formulæ are to apply.

26. *An Electromotive Impulse.*  $m=1$ .—Returning to the case of impressed electric force, let in a spherical portion of an infinite dielectric a uniform field of impressed force act momentarily. We know the result of the continued application of the force. We have, then, to imagine it cancelled by an oppositely directed force, starting a little later. Let  $t_1$  be the time of application of the real force, and let it be a small fraction of  $2a/r$ , the time the travelling shell takes to traverse any point. The result is evidently a shell of depth  $vt_1$  at  $r=vt+a$ , in which the electromagnetic field is the same as in the case of continued application of the force, and a similar shell situated at  $r=vt-a$ , in which  $\mathbf{H}$  is negative. Within this inner shell there is no  $\mathbf{E}$  or  $\mathbf{H}$ . But between the two thin shells just mentioned there is a diffused disturbance, of weak intensity, which is due to the sphericity of the waves, and would be non-existent were they plane waves. In fact, at time  $t=t_1$ , when the initial disturbance  $\mathbf{H}=f_1\nu/2\mu_0v$  has extended itself a small distance  $vt_1$  on each side of the surface of the sphere, there is a radial component  $\mathbf{F}$  at the surface itself, since, by (150),

$$\mathbf{F}_a = f_1 \cos \theta \left( \frac{vt}{a} - \frac{v^3 t^3}{6a^3} \right), \quad . \quad . \quad . \quad (180)$$

so that the sudden removal of  $f_1$  leaves two waves which do not satisfy the condition  $\mathbf{E} = \mu_0 v \mathbf{H}$  at their common surface of contact. On separation, therefore, there must be a residual disturbance between them. The discontinuity in  $\mathbf{E}$  at the moment of removing  $f_1$  is abolished by instantaneous assumption of the mean value, but it is impossible to destroy the radial displacement which joins the two shells at the moment they separate. Put on  $f_1$  when  $t=0$ , then  $-f_1$  at time  $t_1$  later. The  $\mathbf{H}$  at time  $t$  due to both is by (142),

$$\mathbf{H} = \frac{f_1 \nu}{4\mu_0 v r^2} v^2 (t_1^2 - 2tt_1); \quad . \quad . \quad . \quad . \quad (181)$$

which, when  $t_1$  is infinitely small, becomes

$$H = - \frac{f_1 v t t_1 r^2}{2 \mu_0 v r^2} . . . . . (182)$$

First of all, at a point distant  $r$  from the centre, comes the primary disturbance or head,

$$H = \frac{f_1 v a}{2 \mu_0 v r} , . . . . . (183)$$

when  $vt = r - a$ , lasting for the time  $t_1$ . It is followed by the diffused negative disturbance, or tail, represented by (182), lasting for the time  $2a/v$ . At its end comes the companion to (182), its negative, when  $vt = r + a$ , lasting for time  $t_1$ , after which it is all over. This description applies when  $r > a$ . If  $r < a$ , the interval between the beginning and end of the  $H$  disturbance is only  $2r/v$ . From the above follows the integral solution expressing the effect of  $f_1$  varying in any manner with the time.

27. *Alternating Impressed Forces.*—If the impressed force in the sphere, or wherever it may be, be a sinusoidal function of the time, making  $p^2 = -n^2$ , if  $n = 2\pi \times$  frequency, the complete solutions arise from (132) to (135) so immediately that we can almost call them the complete solutions. Of course in any case in which we have developed the connexion between the impressed force and the flux, say  $e = ZC$ , or  $C = Z^{-1}e$ , when  $Z$  is the resistance operator, we may call this equation *the solution in the sinusoidal case*, if we state that  $p^2$  is to mean  $-n^2$ . But there is usually a lot of work needed to bring the solution to a practical form. In the present instance, however, there is scarcely any required, because  $u$  and  $w$  are simple functions of  $qa$ , and  $q^2$  is real. The substitution  $p^2 = -n^2$  in  $u$  results in a real function of  $nr/r$ , and in  $w$  in a real function  $\times (-1)^{\frac{1}{2}}$ . Thus:—

$$\left. \begin{aligned} u_1 &= \cos \frac{nr}{r} - \frac{r}{nr} \sin \frac{nr}{v} \\ w_1 &= i \left( \sin \frac{nr}{r} + \frac{r}{nr} \cos \frac{nr}{r} \right) \end{aligned} \right\} , . . . . . (184)$$

$$\left. \begin{aligned} u_2 &= \left( 1 - \frac{3r^2}{n^2 r^2} \right) \cos \frac{nr}{r} - \frac{3r}{nr} \sin \frac{nr}{v} \\ w_2 &= i \left\{ \left( 1 - \frac{3r^2}{n^2 r^2} \right) \sin \frac{nr}{r} + \frac{3r}{nr} \cos \frac{nr}{r} \right\} \end{aligned} \right\} . . . . . (185)$$

In the case  $m=1$ , if  $(f_1) \cos nt$  is the form of  $f_1$ , so that

( $f_1$ ) represents the amplitude, we find, writing this case fully because it is the most important :—

$$\begin{aligned} (\text{out}) \quad H &= \frac{(f_1)va}{\mu_0 vr} \left( \cos - \frac{v}{na} \sin \right) \frac{na}{v} \cdot \left( \cos - \frac{v}{nr} \sin \right) \left( \frac{nr}{v} - nt \right) \Bigg\} \\ (\text{in}) \quad H &= \frac{(f_1)va}{\mu_0 vr} \left( \cos - \frac{v}{nr} \sin \right) \frac{nr}{v} \cdot \left( \cos - \frac{v}{na} \sin \right) \left( \frac{na}{v} - nt \right) \Bigg\} \end{aligned} \quad (185 a)$$

$$\begin{aligned} (\text{out}) \quad F &= -\frac{2(f_1)va\mu}{nr^2} \left( \cos - \frac{v}{na} \sin \right) \frac{na}{v} \cdot \left( \sin + \frac{r}{nr} \cos \right) \left( \frac{nr}{v} - nt \right) \Bigg\} \\ (\text{in}) \quad F &= -\frac{2(f_1)va\mu}{nr^2} \left( \cos - \frac{r}{nr} \sin \right) \frac{nr}{v} \cdot \left( \sin + \frac{r}{na} \cos \right) \left( \frac{na}{v} - nt \right) \Bigg\} \end{aligned} \quad (185 b)$$

$$\begin{aligned} (\text{out}) \quad E &= \frac{(f_1)av}{r} \left( \cos - \frac{v}{na} \sin \right) \frac{na}{v} \cdot \left\{ \left( 1 - \frac{v^2}{n^2 r^2} \right) \cos \right. \\ &\quad \left. - \frac{v}{nr} \sin \right\} \left( \frac{nr}{v} - nt \right) \Bigg\} \\ (\text{in}) \quad E &= -\frac{(f_1)av}{r} \left\{ \left( 1 - \frac{v^2}{n^2 r^2} \right) \sin + \frac{v}{nr} \cos \right\} \frac{nr}{v} \cdot \left( \sin \right. \\ &\quad \left. + \frac{r}{na} \cos \right) \left( \frac{na}{v} - nt \right) \Bigg\} \end{aligned} \quad (185 c)$$

It is very remarkable on first acquaintance that the impressed force produces no external effect at all when

$$u_a = 0, \quad \text{or} \quad \tan \frac{na}{v} = \frac{na}{v}.$$

For the impressed force may be most simply taken to be a uniform field of intensity ( $f_1$ )  $\cos nt$  in the sphere of radius  $a$  acting parallel to the axis, and it looks as if external displacement must be produced. Of course, on acquaintance with the reason, the fact that the solution is made up of two sets of waves, those outward from the lines of vorticity and those going inward, and then reflected out, the mystery disappears.

To show the positive and negative waves explicitly, we may write the first of (185 a) in the form

$$\begin{aligned} (\text{out}) \quad H &= \frac{(f_1)av}{2\mu_0 vr} \left[ \left\{ \left( 1 - \frac{v^2}{n^2 ar} \right) \cos + \left( \frac{v}{na} + \frac{v}{nr} \right) \sin \right\} \left( nt - \frac{n(a+r)}{v} \right) \right. \\ &\quad \left. + \left\{ \left( 1 + \frac{v^2}{n^2 ar} \right) \cos + \left( \frac{v}{nr} - \frac{v}{na} \right) \sin \right\} \left( nt + \frac{n(a-r)}{r} \right) \right], \end{aligned} \quad (185 d)$$

the second line showing the primary wave out, the first the reflected wave\*. Exchange  $a$  and  $r$  within the [ ] to obtain

\* In reference to this formula (185 d), and the corresponding ones for other values of  $m$ , it is not without importance to know that a very

400 Mr. O. Heaviside on *Electromagnetic Waves*, and the  
the internal H. The disturbance, at the surface, of the primary  
wave going both ways is, from  $t=0$  to  $2a/r$ ,

$$\frac{(f_1)\nu}{2\mu_0 v} \left\{ \cos nt + \frac{v^2}{n^2 ar} (\cos nt - 1) \right\} \quad (185 e)$$

The amplitude due to both waves is

$$\frac{(f_1)\nu}{\mu_0 v} u_a \left( 1 + \frac{v^2}{n^2 a^2} \right)^{\frac{1}{2}} \quad (185 f)$$

The outward transfer of energy per second per unit-area  
at any distance  $r$  is  $EH/4\pi$ . In the  $m$ th system this is

$$\frac{EH}{4\pi} = -\frac{(f_m)^2 a^2 (\nu Q_m)^2}{4\pi (\mu_0 v)^2 r} \cdot \frac{u_a^2}{cnr} \cdot \{u' \sin - (-iv') \cos\} nt \cdot \{u \cos + (-iv) \sin\} nt, \quad (186)$$

where  $m$  is supposed odd, whilst  $u$  and  $-iv$  are the real  
functions of  $nr/r$  obtained in the same way as (184). The  
mean value of the  $t$  function is, by the conjugate property of  
 $u$  and  $v$ , equation (114),

$$= -n/2r.$$

Using this, and integrating (186) over the complete surface  
of radius  $r$ , giving

$$\iint (\nu Q_m)^2 dS = \frac{4\pi r^2 m(m+1)}{2m+1}, \quad (187)$$

we find the mean transfer of energy outward per second  
through any surface enclosing the sphere to be

$$\frac{m(m+1)}{2(2m+1)} \frac{(f_m')^2 u_a^2 a^2}{\mu_0 v}, \quad (188)$$

if  $(f_m)\nu Q_m \cos nt$  is the vorticity of the impressed force.

slight change suffices to make (185 d) represent the solution from the first  
moment of starting the impressed force. Thus, let it start when  $t=0$ ,  
and let the  $f_1$  in equation (185 d) be  $(f_1) \cos nt$ . Effect the two integrations  
thus,

$$\frac{f_1}{q} = (f_1) \frac{v}{n} \sin nt, \quad \frac{f_1}{q^2} = (f) \frac{v^2}{n^2} (1 - \cos nt),$$

vanishing when  $t=0$ , and then operate with the exponentials, and we  
shall obtain (185 d) thus modified. To the first line must be added

$$\frac{(f_1)\nu a}{2\mu_0 vr} \frac{v}{n^2 ar},$$

and to the second line its negative. Thus modified, (185 d) is true from  
 $t=0$ , understanding that the second line begins when  $t=(r-a)/v$ , and the  
first when  $t=(r+a)/v$ . The first of (185 a) is therefore true up to distance  
 $r=vt-2a$ , when this is positive. In the shell of depth  $2a$  beyond, it fails.



In the case  $m=1$ , the waste of energy per second is

$$\frac{(f_1)^2 a^2 u_a^2}{3\mu_0 v}, \dots \dots \dots (189)$$

due to the uniform alternating field of impressed force of intensity

$$(f_1) \cos nt$$

within the sphere.

In reality, the impressed force must have been an infinitely long time in operation to make the above solutions true to an infinite distance, and have therefore already wasted an infinite amount of energy. If the impressed force has been in operation any finite time  $t$ , however great, the disturbance has only reached the distance  $r=vt+a$ . Of course the solutions are true, provided we do not go further than  $r=vt-a$ . We see, therefore, that the real function of the never-ceasing waste of energy is to set up the sinusoidal state of  $\mathbf{E}$  and  $\mathbf{H}$  in the boundless regions of space to which the disturbances have not yet reached. The above outward waves are the same as in Rowland's solutions\*. Here, however, they are explicitly expressed in terms of the impressed forces causing them.

$u_a=0$  makes the external field vanish when  $m$  is odd; and  $v_a=0$  when  $m$  is even; that is, when the sinusoidal state has been assumed. It takes only the time  $2a/v$  to do this, as regards the sphere  $r=a$ ; the initial external disturbance goes out to infinity and is lost. This vanishing of the external field happens whatever may be the nature of the external medium away from the sphere, except that the initial external disturbance will behave differently, being variously reflected or absorbed according to circumstances.

28. *Conducting Medium.*  $m=1$ .—Now consider the same problem in an infinitely extended conductor of conductivity  $k$ . We may remark at once that, unless the conductivity is low, the solution is but little different from what it would be were the conductor not greatly larger than the spherical portion within it on whose surface lie the vortex lines of the impressed force, owing to the great attenuation suffered by the disturbances as they progress from the surface. In a similar manner, if the sphere be large, or the frequency of alternations great, or both, we may remove the greater part of the interior of the sphere without much altering matters.

We have now

$$q=(4\pi\mu_0 kp)^{\frac{1}{2}}=(1+i)x,$$

if

$$x=(2\pi\mu_0 kn)^{\frac{1}{2}}. \dots \dots \dots (190)$$

\* In paper referred to in § 18.

The realization is a little troublesome on account of this  $p^{\frac{1}{2}}$ . The result is that the uniform alternating field of impressed force of intensity

$$(f_1) \cos nt,$$

gives rise to the internal solution

$$(\text{in}) \quad H = \left( \frac{\pi k}{2\mu_0 n} \right)^{\frac{1}{2}} \frac{(f_1)va}{r} \{ (A+B) \cos nt + (A-B) \sin nt \}, \quad (191)$$

where A and B are the functions of  $r$  expressed by

$$A = e^{x(r-a)} \left[ \left( 1 + \frac{1}{2xa} - \frac{1}{2xr} \right) \cos + \left( \frac{1}{2xr} - \frac{1}{2xa} + \frac{2}{2xr \cdot 2xa} \right) \sin \right] x(a-r) \\ + e^{-x(r+a)} \left[ \left( 1 + \frac{1}{2xa} + \frac{1}{2xr} \right) \cos - \left( \frac{1}{2xa} + \frac{1}{2xr} + \frac{2}{2xr \cdot 2xa} \right) \sin \right] x(a+r); \quad (192)$$

$$B = e^{x(r-a)} \left[ \left( \frac{1}{2xr} - \frac{1}{2xa} + \frac{2}{2xr \cdot 2xa} \right) \cos - \left( 1 - \frac{1}{2xr} + \frac{1}{2xa} \right) \sin \right] x(a-r) \\ - e^{-x(r+a)} \left[ \left( \frac{1}{2xr} + \frac{1}{2xa} + \frac{2}{2xr \cdot 2xa} \right) \cos + \left( 1 + \frac{1}{2xr} + \frac{1}{2xa} \right) \sin \right] x(a+r). \quad (193)$$

Equation (191) showing the internal H, the external is got by exchanging  $a$  and  $r$  in the functions A and B.

Now  $va$  is easily made large, in a good conductor; then, anywhere near the boundary, ( $r=a$ ), we have

$$\left. \begin{aligned} A &= e^{-x(a-r)} \cos x(a-r), \\ -B &= e^{-x(a-r)} \sin x(a-r), \end{aligned} \right\}; \quad \dots \quad (194)$$

and (191) becomes

$$(\text{in}) \quad H = \left( \frac{\pi k}{\mu_0 n} \right)^{\frac{1}{2}} \frac{(f_1)av}{r} e^{-x(a-r)} \cos \left\{ nt - x(a-r) - \frac{\pi}{4} \right\}. \quad (195)$$

The wave-length  $\lambda$  is

$$\lambda = \left( \frac{2\pi}{\mu_0 kn} \right)^{\frac{1}{2}}. \quad \dots \quad (196)$$

Thus, in copper, a frequency of 1600 to 1700 makes  $\lambda = 1$  centim. Both  $\lambda$  and the attenuation-rate depend inversely on the square roots of the inductivity, conductivity, and frequency, whereas the amplitude varies directly as the square root of the conductivity, and inversely as the square roots of the others.

To verify that very great frequency ultimately limits the disturbance to the vortex line of  $e$  when there is but one, we may use the last solution to construct that due to a sheet of impressed force

$$\cos nt \sum e_m Q_m$$

acting radially on the surface of the sphere. Thus,

$$(in) \ H = \left( \frac{\pi k}{\mu_0 n} \right)^{\frac{1}{2}} \sum \frac{e_m \nu Q_m}{r} \epsilon^{-x(a-r)} \cos \left\{ nt - x(a-r) - \frac{\pi}{4} \right\} \quad (197)$$

when  $xa$  is very great. When the vorticity is confined to one line of latitude,  $H$  in (197) vanishes everywhere except at the vortex line. But a further approximation is required, or a different form of solution, to show the disturbance round the vortex line explicitly, *i. e.* when  $n$  is great, though not infinitely great.

29. *A Conducting Dielectric.  $n=1$ .*—Here, if  $k$  is the conductivity,  $c$  the permittivity, and  $\mu_0$  the inductivity, let

$$q = (4\pi\mu_0 kp + \mu_0 cp^2)^{\frac{1}{2}} = n_1 + n_2 i, \quad . \quad . \quad (198)$$

when  $p = ni$ . Then  $n_1$  and  $n_2$  will be given by

$$\left. \begin{aligned} n_1^2 &= \frac{n^2}{2v^2} \left[ \left\{ 1 + \left( \frac{4\pi k}{cn} \right)^2 \right\}^{\frac{1}{2}} - 1 \right] \\ n_2^2 &= \frac{n^2}{2v^2} \left[ \left\{ 1 + \left( \frac{4\pi k}{cn} \right)^2 \right\}^{\frac{1}{2}} + 1 \right] \end{aligned} \right\} \quad . \quad (199)$$

Using this  $q$  in the general external  $H$  solution, but ignoring the explicit connexion with the impressed force, we shall arrive at

$$\begin{aligned} (out) \ H &= \frac{C_0 \nu}{r} \epsilon^{-n_1 r} \left[ \left( 1 + \frac{n_1}{r(n_1^2 + n_2^2)} \right) \cos \right. \\ &\quad \left. - \frac{n_2}{r(n_1^2 + n_2^2)} \sin \right] (n_2 r - nt), \quad . \quad . \quad (200) \end{aligned}$$

where  $C_0$  is an undetermined constant, depending upon the magnitude of the disturbance at  $r=a$ . So far as the external solution goes, however, the internal connexions are quite arbitrary save in the periodicity and confinement to producing magnetic force proportional in intensity to the cosine of the latitude. The solution (200) may be continued unchanged as near to the centre as we please. Stopping it anywhere, there are various ways of constructing complementary distributions in the rest of space, from which (200) is excluded.

$n_1$  is zero when  $k=0$ . We then have the dielectric solution, with  $n_2 = n/v$ . On the other hand,  $c=0$  makes

$$n_1 = n_2 = (2\pi\mu_0 kn)^{\frac{1}{2}} = \nu,$$

as in § 28. The value of  $n_1^2 + n_2^2$  is

$$\frac{n^2}{v^2} \left( 1 + \left( \frac{4\pi k}{cn} \right)^2 \right)^{\frac{1}{2}} = \frac{n^2}{v^2} \left( 1 + \left( \frac{4\pi k \mu_0 v^2}{n} \right)^2 \right)^{\frac{1}{2}} \quad . \quad (201)$$

Enormously great frequency brings us to the formulæ of the non-conducting dielectric, with a difference, thus :  $n_1$  and  $n_2$  become

$$n_1 = 2\pi k \mu_0 v, \quad n_2 = n/v, \quad . \quad . \quad . \quad (202)$$

when  $4\pi k/\omega n$  is a small fraction. The attenuation due to conductivity still exists, but is independent of the frequency. We have now

$$(\text{out}) \quad H = \frac{C_0 \nu}{r} \epsilon^{-n_1 r} \left( \cos - \frac{v}{nr} \sin \right) \left( \frac{nr}{v} - nt \right), \quad (203)$$

differing from the case of no conductivity only in the presence of the exponential factor.

It is, however, easily seen by the form of  $n_1$  in (202) that in a good conductor the attenuation in a short distance is very great, so that the disturbances are practically confined to the vortex lines of the impressed force, where the  $H$  disturbance is nearly the same as if the conductivity were zero, as before concluded. It follows that the initial effect of the sudden introduction of a steady impressed force in the conducting dielectric is the emission from the seat of its vorticity of waves in the same manner as if there were no conductivity, but attenuated at their front to an extent represented by the factor  $\epsilon^{-n_1 r}$ , with the (202) value of  $n_1$ , in addition to the attenuation by spreading which would occur were the medium non-conducting. This estimation of attenuation applies at the front only.

30. *Current in Sphere constrained to be uniform.*—Let us complete the solution (200) of § 29 by means of a current of uniform density parallel to the axis within the sphere of radius  $a$ , beyond which (200) is to be the solution. This will require a special distribution of impressed force, which we shall find. Equation (200) gives us the normal component of electric current at  $r=a$ , by differentiation. Let this be  $\Gamma \cos \theta$ . Then  $\Gamma$  is the density of the internal current. The corresponding magnetic field must have the boundary-value according to (200), and vary in intensity as the distance from the axis, its lines being circles centred upon it, and in planes perpendicular to it. Thus the internal  $\mathbf{H}$  is also known. The internal  $\mathbf{E}$  is fully known too, being  $k^{-1}\Gamma$  in intensity and parallel to the axis. It only remains to find  $\mathbf{e}$  to satisfy

$$\text{curl} (\mathbf{e} - \mathbf{E}) = \mu \dot{\mathbf{H}}, \quad . \quad . \quad . \quad . \quad (3) \text{ bis}$$

within the sphere, and at its boundary (with the suitable surface interpretation), as it is already satisfied outside the sphere. The simplest way appears to be to first introduce a uniform



field of  $\mathbf{e}$  parallel to the axis, of such intensity  $e_1$  as to neutralize the difference between the tangential components of the internal and external  $\mathbf{E}$  at the boundary, and so make continuity there in the force of the field; and next, to find an auxiliary distribution  $\mathbf{e}_2$ , such that

$$\text{curl } \mathbf{e}_2 = \mu \dot{\mathbf{H}},$$

and having no tangential component on the boundary. This may be done by having  $\mathbf{e}_2$  parallel to the axis, of intensity proportional to

$$(a^2 - r^2) \sin \theta.$$

The result is that the internal  $\mathbf{H}$  is got from the external by putting  $r=a$  in (200) and then multiplying by  $r/a$ ;  $\mathbf{E}$  from the internal  $\mathbf{H}$  by multiplying by  $(2\pi r \sin \theta)^{-1}$ ;  $\mathbf{e}_1$  from the difference of the tangential components  $\mathbf{E}$  outside and inside is given by

$$\begin{aligned} e_1 = \frac{C_0}{4\pi a^2} \epsilon^{-n_1 a} \left\{ k^2 + \left( \frac{cn}{4\pi} \right)^2 \right\}^{-1} & \left[ \left\{ k \left( 3 + n_1 a + \frac{3n_1}{a(n_1^2 + n_2^2)} \right) \right. \right. \\ & \left. \left. + \frac{cn}{4\pi} \left( n_2 a - \frac{3n_2}{a(n_1^2 + n_2^2)} \right) \right\} \cos(n_2 r - nt) \right. \\ & - \left\{ k \left( \frac{3n_2}{a(n_1^2 + n_2^2)} - n_2 a \right) \right. \\ & \left. \left. + \frac{cn}{4\pi} \left( 3 + n_1 a + \frac{3n_1}{a(n_1^2 + n_2^2)} \right) \right\} \sin(n_2 r - nt) \right]. \quad (204) \end{aligned}$$

Finally, the auxiliary force has its intensity given by

$$\begin{aligned} e_2 = \mu_0 n C_0 \frac{a^2 - r^2}{2a^2} \epsilon^{-n_1 a} & \left\{ \left( 1 + \frac{n_1}{a(n_1^2 + n_2^2)} \right) \sin \right. \\ & \left. + \frac{n_2}{a(n_1^2 + n_2^2)} \cos \right\} (n_2 a - nt). \quad (205) \end{aligned}$$

A remarkable property of this auxiliary force, which (or an equivalent) is absolutely required to keep the current straight, is that it does no work on the current, on the average; the mean activity and waste of energy being therefore settled by  $e_1$ .

L. *The Thermodynamics of Cryohydrates.* By J. PARKER, B.A., late Scholar of St. John's College, Cambridge\*.

DR. GUTHRIE has found that when a dilute saline solution, formed by dissolving a salt in water or any other liquid, is subjected to reduction of temperature, the ice formed is perfectly fresh, until a certain temperature is reached, which depends only on the natures of the salt and liquid employed, and not on their relative proportions. The solution then freezes, without further reduction of temperature, into a solid homogeneous brine. Dr. Guthrie has also found that the final solution thus obtained, which he calls a "cryohydrate," is of the same strength whatever may have been the strength of the dilute solution with which we started.

Again, he has found that the lowest temperature obtainable by means of a "cryogen," *i.e.* a freezing mixture of salt and ice, is the freezing-point of the corresponding cryohydrate.

These experimental results have been obtained at atmospheric pressure, but we shall suppose them to be true at any pressure.

We at once infer that at the freezing-point of the cryohydrate, the salt and pure ice are neutral to each other; in other words, a saturated solution is then in stable equilibrium with pure ice. There is, therefore, only one stable solution possible at this temperature, *viz.* the cryohydrate itself.

At any temperature above the freezing-point of the cryohydrate, and below the freezing-point of the pure liquid, there may be any number of stable solutions, the strongest being a saturated solution, and the weakest in stable equilibrium with pure ice.

Analytically, let  $\phi$ ,  $\psi$  be the thermodynamic potentials at constant pressure of unit weights of the salt and ice, respectively, at any temperature between the freezing-points of the cryohydrate and of the pure liquid.

Also let the weakest stable solution contain a weight  $m_1$  of the salt, and a weight  $m_2$  of the liquid. Its thermodynamic potential will be

$$\Phi = m_1 f_1 + m_2 f_2,$$

where  $f_1$ ,  $f_2$  are homogeneous in  $m_1$  and  $m_2$  and of degree 0.

But the solution is in stable equilibrium with pure ice, and not with the salt; hence;

$$f_1 < \phi, \quad f_2 = \psi.$$

\* Communicated by the Author.



Then we have by (3),

$$\Phi = \Phi_1.$$

Also

$$\Phi = E(U - tS) + pv, \quad \Phi_1 = E(U_1 - tS_1) + pv_1,$$

where  $E$  is the mechanical equivalent of heat.

Again, if the salt and ice be placed in contact with each other, and the temperature kept slightly above the freezing-point of the cryohydrate, the liquid cryohydrate will be formed, and if  $Q$  is the heat absorbed during the process, we must have, since  $\Phi = \Phi_1$ ,

$$Q = t.(S - S_1) \dots \dots \dots (4)$$

If we move to a consecutive point ( $p + \delta p$ ,  $t + \delta t$ ) on the freezing line, an indefinitely small quantity of ice (or salt) may separate out from the cryohydrate; but whether it does so or not, the changes in  $\Phi$ ,  $\Phi_1$  will be respectively

$$\left. \begin{aligned} \delta\Phi &= v.\delta p - S.\delta t \\ \delta\Phi_1 &= v_1.\delta p - S_1.\delta t \end{aligned} \right\}.$$

But by (3)

$$\delta\Phi = \delta\Phi_1;$$

hence

$$(v - v_1).\delta p = (S - S_1).\delta t,$$

and, therefore, by (4),

$$\frac{Q}{t} = (v - v_1) \cdot \frac{\delta p}{\delta t} \dots \dots \dots (5)$$

We may notice, generally, that the freezing-point of the cryohydrate is depressed or raised by pressure, according as the liquid employed expands or contracts in the act of freezing.

In particular, the freezing-point of the cryohydrate of an aqueous solution of sodium chloride is lowered by pressure by almost exactly the same amount as the freezing-point of pure water.

We will now give reasons for believing that the composition of a cryohydrate is independent of the pressure. For suppose, if possible, that along the freezing line of the cryohydrate,  $h$  increases with  $p$ .

At any pressure  $p$ , let the temperature of the cryohydrate be reduced below its freezing-point, and then let the pressure increase if  $\frac{\delta p}{\delta t}$  is negative along the freezing line, but if

$\frac{\delta p}{\delta t}$  is positive, let the pressure decrease.

Referring to the diagram we see that, sooner or later, the



solid cryohydrate must melt into an unstable solution. The improbability of this event leads us to conclude that  $h$  cannot increase with  $p$ . Similarly we may reason that  $h$  cannot decrease as  $p$  increases. A cryohydrate must therefore be regarded as a body of *definite chemical composition*, and not as a mere solution in which the proportions of salt and ice depend upon various accidents.

This property of a cryohydrate has already been noticed by Dr. Guthrie, but he has not examined the effects of pressure.

Again, Duhem has shown that when a dilute solution, formed by dissolving any salt in water, is reduced in temperature until the pure ice appears, then increase of pressure causes some of the ice to melt, or, in analytical language,  $\frac{dh}{dp}$  is negative for the weakest stable solution.

If, however, the solution is formed by dissolving the salt in a liquid, like acetic acid, which contracts whilst freezing, then  $\frac{dh}{dp}$  is positive.

As  $h$  is constant along the freezing line of the cryohydrate, it follows that in the former case  $\frac{\delta p}{\delta t}$  is negative, and in the latter case positive.

It is now evident that when a salt is dissolved in water, the cryohydrate expands in the act of freezing, and its melting-point is lowered by pressure, whereas, when a salt is dissolved in a liquid, like acetic acid, which contracts whilst freezing, the cryohydrate also contracts whilst freezing, and its melting-point is raised by pressure.

LI. *Experiments with Soap-bubbles.* By C. V. BOYS, A.R.S.M., Demonstrator of Physics at the Science Schools, South Kensington\*.

[Plate II.]

**T**HOUGH none of the experiments I am about to describe depend upon any property of a soap-film which is not perfectly well known and understood, yet they serve to illustrate in a striking and beautiful manner the behaviour of bubbles under special circumstances, and so as lecture-experiments simply I hope they may be considered worthy of the attention of the Physical Society.

\* Communicated by the Physical Society : read April 14, 1888.

Everyone is familiar with the fact that a soap-bubble may be supported or even struck by a piece of baize or wool without coming into real contact with the material; it is also well known that two bubbles supported on the pipes from which they are blown, or on rings, may be pressed or knocked together with such violence as to materially alter their shape, and yet they do not come into real contact; there is a film of air between them which they are unable to squeeze out. This film, though thin to ordinary tests, is so thick that the colours of Newton's rings are only seen when one of the bubbles is very small, so that the air is squeezed out the more readily. If the pressure is increased so as to make a real contact, the bubbles both instantly burst. That this pressure may be made great before the true contact takes place will be shown in a variety of ways hereafter; but the following simple experiment makes it very evident that the air-film will prevent the contact of two soap-films that are pressed together.

*Exp. 1.*—Blow a bubble about 9 cm. in diameter, and place it on a ring with a diameter of about 7 cm. This bubble may be pulled or pushed through the ring by means of a smaller wire ring which serves as a handle. (See 'Nature,' 1871, p. 395.) It may be so adjusted that the weight of the ring will not pull it through. Then a ring larger than the bubble, carrying a plane film, can be used to push it up and down through the ring, and yet the two films do not touch (fig. 1, Pl. II.).

Bearing this fact in mind, that two bubbles may press one another without true contact, I hoped to be able to blow and detach one bubble within another, and let it roll about within the larger bubble. This, however, is made difficult by the accumulation of a small quantity of solution at the bottom of each, the weight of which is able immediately to press through the air-film between them and so cause both bubbles to burst. However, the experiment can be performed in the following manner:—

*Exp. 2.*—Blow a bubble on the lower side of the same ring that was used in Exp. 1, and if a large drop does not remain hanging to the bubble slowly apply solution to any part until as great a drop as can safely be carried has accumulated. Then pass the end of the pipe through the upper side of the bubble, and blow another inside, but take care in this case to have no excess of liquid. When the inner bubble is about twice as large as the outer one was at first, remove the pipe with a rapid movement. The inner one will now fall gently and rest within the outer one, the heavy drop pulling the thick part of the outer bubble out of reach



Fig. 1.



Fig. 6.

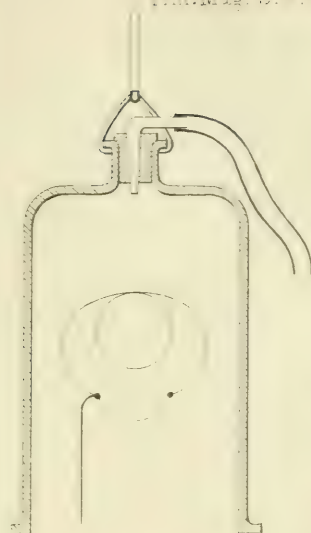
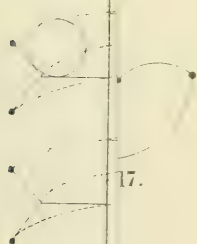


Fig. 9.



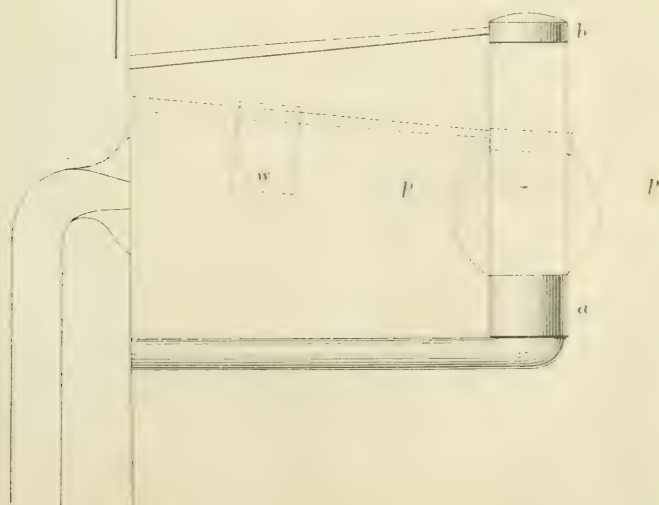
Fig. 10.



17.



Fig. 18.







of the inner one. The air of the outer bubble may then be withdrawn until the space between the highest point of the two bubbles is no more than two or three millimetres (fig 2, Pl. II.).

The great pressure which the air-film will carry is well shown by the next experiment, which, moreover, is more easily carried out than the last.

*Exp. 3.*—Proceed as in the last experiment, but instead of making a large drop on the first bubble, hang on a moistened ring of wire rather smaller than the fixed ring. This ring should be weighted until it pulls the bubble so much out of shape that a tangent to the curve at the points where the film meets the hanging ring makes an angle of  $20^\circ$  or  $30^\circ$  with the plane of the ring (fig. 3, Pl. II.). A bubble may then be blown inside and allowed to drop, when it will be found to rest on the conical seat provided by the outer bubble, while the heavy drops of liquid are kept apart, and thus there is no fear of contact (fig. 4). These drops may now be both removed with the end of the blowpipe; then, if the lower ring is pulled down slowly, it will be found that the inner bubble is being squeezed out of shape until it becomes a beautiful oval, while the outer bubble shows the effect of the pressure by a corresponding enlargement (fig. 5). If the lower ring is pulled down still further, the outer bubble is simply pulled in half, and the inner one, often unbroken, gently floats away. This shows that contact was not made, as in that case both would be immediately broken. If, however, instead of pulling the ring too far, it is held in the position shown in fig. 5, it will be found that it is possible to swing the pair of bubbles round and round, and yet in spite of this violent treatment the bubbles refuse to touch one another. Or, if the lower ring is cautiously inclined and pulled away, the outer bubble will peel off it and remain attached to the upper one only. The two bubbles will now be spherical again, but there will be no heavy drop as in fig. 2. The air of the outer bubble may be withdrawn as before, until the two bubbles are barely separate.

This experiment, and many of those that follow, may be made more beautiful by using for the inner bubble a solution strongly coloured by fluoresceine, or still better by uranine (for the knowledge of which I am indebted to Mr. Madan); then, if sunlight, electric or magnesium light is thrown on to the bubbles, the inner one appears a brilliant green, while the outer one remains clear as before.

The power of the surface-tension to do work is demonstrated by blowing a large bubble below the ring and hanging on the weighted ring. If now a very small ring, a

centimetre or more in diameter, is placed on that part of the bubble which is stretched across either ring, and then the part within the small ring is made to burst, the air will escape through the small hole and the heavy ring will be lifted until it comes in contact with the upper one. If the film over the whole of the heavy ring is burst instead, the ring is pulled up so suddenly that it is difficult to follow it with the eye, and it strikes the upper ring with such violence that the noise is loud enough to be heard across a large room.

A suspended ring affords a simple and accurate means of measuring the surface-tension of the soap-film. A plane film is formed across a fixed horizontal ring and a light smaller ring is attached to the plane film, which is then broken within the smaller ring so as to leave an annular film only. Weights are then hung on to the suspended ring until the angle between the film and the plane of this ring approaches  $90^\circ$ . At this point equilibrium becomes unstable, and the lower ring falls away, but now both rings will be found to carry plane films, though the moment before neither did. On repeating the experiment a few times it will often be found possible to use such a weight that the ring will hang for some time, but will gradually sink, while the angle referred to above will approach more and more nearly to  $90^\circ$  as the surface-tension of the film diminishes; and thus the exact surface-tension at the particular moment of separation may be found by dividing weight of the ring and attached moisture by twice its circumference.

*Exp. 4.*—Bubbles blown with coal-gas are lighter than air and rise. If therefore an inner bubble is blown with such a mixture of air and gas as to rise, it will rest against the upper side of the outer bubble, where there are no heavy drops but where the films are thinnest and cleanest (fig. 6). A pair of bubbles blown in this way will sometimes last an hour when exposed to the air of the room. The inner bubble may be gradually enlarged by blowing in gas until the outer one can barely withstand the pull. The forms assumed under these circumstances are extremely graceful, and their beauty is increased by the play of colours on the two bubbles which the multiple reflexions seem to intensify. If, when the inner bubble is not too large, as in fig. 6, a little gas is gently let into the outer bubble, it is possible to so adjust the mixture of gas and air that the inner will float either near the top or near the bottom of the outer bubble, or about the middle, as may be desired (fig. 8). If under these conditions the bubbles are left undisturbed, the richer gas above the inner bubble will diffuse into the poorer and heavier gas below, and the

bubble will slowly rise or fall, according to the relative quantities of gas and air. The diffusion through the film is well shown in the next experiment.

*Exp. 5.*—Blow a pair of bubbles, as shown in fig. 6, but make the inner bubble only just light enough to rest against the top of the outer one. Lower a bell-jar over all, and pass a stream of gas into the bell-jar by means of a tube passing through the top. As the air is gradually driven down, the outer bubble begins to feel the want of buoyancy, and gradually settles down, as shown in fig. 9. After a short time, the effect of the diffusion through both bubbles tending to enrich the gas of the outer bubble is made evident by the gentle descent of the inner bubble. If the jar is raised quickly, and a little air is blown into the outer bubble, it is possible to again cause the inner one to rise and float against the top of the outer one as before. The bell-jar may be lowered and the process repeated until the outer bubble is so large that the ring is unable to support its weight when in an atmosphere of coal-gas.

The very rapid diffusion of a vapour which will mix with the solution of which the film is made is easily shown.

*Exp. 6.*—Into a large inverted bell-jar pour a small quantity of ether, or to fill the jar with the vapour quickly wet a piece of blotting-paper with ether and stand it on edge in the jar. Remove the paper, then blow a bubble and drop it into the jar. The bubble will rest on the ether vapour as on carbonic anhydride, and while floating the most violent agitation of the colours of the film will be seen. The bubble does not remain floating long at the same level; it gradually sinks into denser and denser layers of vapour until it reaches the bottom or breaks on the way. This gradual sinking is due to the penetration into the bubble of the ether vapour, as may be shown as follows:—The bubble may be taken out of the vapour by means of a ring wetted with soap-solution and carried to a flame, when instantly there is a blaze of ether vapour a foot or more in diameter. That the flame is not due to liquid ether condensed on the film is shown by exposing a plane film to the vapour and carrying it to a light in the same way, when no trace of flame will be seen.

*Exp. 7.*—At the end of a wide tube, which has been enlarged at the lower end, blow a large bubble and lower it gently into the vapour of ether, holding the finger at the mouth of the tube. After a few seconds it will be found difficult to remove the bubble by means of the tube, because its weight may have become sufficient to tear it away when buoyed up by the air only. If it is removed successfully it



will hang like a heavy drop; then, on removing the finger, a light may be put to the issuing vapour, which will burn like a bunsen-burner. If, moreover, the bubble full of ether vapour is held in a brilliant light, the shadow will show the ether vapour oozing through the film and falling away in a heavy stream (fig. 10). This experiment shows in succession the floating of an air-bubble on a heavy vapour, rapid diffusion of a soluble vapour through a soap-film, and the power of the surface-tension to force the heavy vapour up a tube fast enough to supply a large flame.

*Exp. 8.*—Blow a bubble with oxygen gas in a jar partly filled with ether vapour; on taking the bubble out of the vapour and carrying it to a light, it will explode with a loud report. Sufficient vapour will penetrate the bubble, even whilst it is being blown, to make the mixture violently explosive.

*Exp. 9.*—The weight of the air is well shown by blowing a bubble with gas on a ring and then trying to blow an air-bubble within it (fig. 11). The inner bubble is then pulled out into a pear-shape, and very soon breaks away from the pipe on account of its great weight.

*Exp. 10.*—If *Exp. 4* be repeated, but instead of a heavy fixed ring a light aluminium one be used instead, to which is tied a long piece of thread which may have a sheet of paper at the end, then the whole combination will float and rise in the air, even though, as in fig. 7, practically the whole of the buoyancy is due to the gas in the inner bubble. In this case the inner bubble is the bag of a balloon, the outer bubble is the netting, and the wire and the things carried by it are the car. In this case the power of the air-film to resist contact of the two films is more evident than ever. If any of the former figures 6, 7, or 8 are carrying a wire ring and thread, as described, it is possible by a suitable pull at the thread to release the pair of bubbles, which float away, one inside the other, until the ceiling brings the experiment to a conclusion.

*Exp. 11.*—If the inner bubble of fig. 6 is made smaller than the ring, then the corresponding experiment to that represented in fig. 5 is shown in fig. 12. The small sphere will always roll to the upper end of the outer bubble, which may be pulled out to the cylindrical form and be inclined either way. This modification of the other experiment was suggested by Mr. Newth, to whom I had shown the previous combinations.

A great many experiments may be shown in which strings of two or more bubbles, filled some with air and some with gas, tend to pull in different directions. Thus an air-bubble



with a gas-bubble blown on the top of it will rise till the gas-bubble breaks against the ceiling, when the air-bubble falls again, and may be sent up as often as desired by the addition of a new gas-bubble; or three bubbles, the lower one of air the upper one of gas, so proportioned that the combination just floats, will remain until the middle bubble is touched with the finger, when the other two immediately go opposite ways. There is no occasion to say more about experiments of this type.

*Exp. 12.*—An experiment which is easily performed shows in a striking way how the air-film resists being broken. If a pair of bubbles are blown as shown in fig. 4, and the vibrating prongs of a large tuning-fork are brought quite close to the line where one bubble rests upon the other, both films will take up the movement of the fork, and a point of light reflected by the two films is seen spread out into a pair of rings, so violent is the motion, yet the films do not touch. It is hardly possible to suppose that the two films remain as close together where the movement occurs as in other parts of the line of support; if they tend to separate they form an exception to the general rule that a vibrating body attracts an object in the immediate neighbourhood. In this case the inner bubble is heavier than the air in the outer one, both because of the weight of the film and the compression of the air within due to its tension. But if the same experiment is tried when the inner bubble is lighter than the air in the outer one, as it may be by holding one of the prongs close to the highest point of the bubbles shown in fig. 6, or when either bubble is heavier or lighter than air, the same result will be found—the bubbles will refuse to touch one another.

Plateau has described (*Statique des liquides*) a number of very beautiful experiments in which wire frames representing the edges of geometrical solids are dipped in soap-solution, after which they are found to carry combinations of films, plane or curved according to the character of the frame. Thus within a triangular prism, when it is removed from the solution, is found a combination of nine plane films which form three troughs meeting along the axis of the prism and a triangular pit at each end.

*Exp. 13.*—A spherical bubble may be dropped into one of these troughs and rolled from end to end, it may be taken out of one trough and dropped into another, or the frame may be held with its axis vertical, when the bubble may be dropped into the triangular pit, where, however, it will not remain long.

The characteristic feature of all the luminar figures is that

there are never more than three surfaces meeting in a line where the angles are always  $120^\circ$ , or more than four lines or six surfaces meeting in a point. Further, the mean curvatures of the films are always zero so long as no air is enclosed. As Plateau mentions in his book, the screw-surface has no mean curvature, therefore if a frame be made out of a helix of wire with its ends connected by wire to a solid axis, such a frame after being dipped will carry a helical surface of soap-film.

*Exp. 14.*—If, instead of a single helix of wire, two helices are fixed to the same axis, but not quite symmetrically, so that in any part the wire of one helix is nearer that above it than the one below, two helical films will not be formed, but there will be a single one in an intermediate position which will be joined to the two wire screws by a pair of conical screw-surfaces, these forming with the true screw-surface a screw-shaped trough down which bubbles may be rolled or up which they may be wound, as water is wound up by a screw-pump (fig. 13). Further, if a series of small bubbles are blown along the helical edge in which the three films meet at angles of  $120^\circ$ , a spiral staircase is made of soap-film, down which a bubble will run one or two steps at a time, and from which it will escape uninjured when it reaches the bottom. Of course bubbles lighter than air, in the same way, will rest against the lower sides of a trough or roll up instead of down the screw-surface.

*Exp. 15.*—One more experiment in which the rolling of bubbles is the chief feature is worth describing. Three rings of wire, seen in section in fig. 14, are joined together by wires, shown dotted, and are carried by a central axis, which may be made to rotate. After this frame has been dipped in the solution of soap, and the three radial planes broken, it is found to carry a circular trough, into which a series of bubbles may be dropped, while at the same time the frame may be kept rotating, so that the bubbles are rolling round and round like marbles on the rim of a solitaire-board. A corresponding frame might possibly be made of light wire, which after dipping would rest on the bubbles in the first frame, thus forming a working model of the ball-bearing. I have not, however, succeeded.

Plateau has mentioned the fact (p. 166) that M. Chautard has found a soap-film a convenient envelope for a gas which is to be tested magnetically. He says that a spherical film above one pole of an electromagnet is visibly disturbed if the gas within has magnetic properties when the exciting current from a battery of 25 to 30 Bunsen's cells is made and broken.

If, instead of a spherical bubble, one of cylindrical form, with its length about three times its diameter, is used, the distortion produced by a small disturbing force is so greatly magnified that, using an electromagnet actuated by five Grove's cells only, not only is the change of form manifest when oxygen is the gas in the bubble, but it is even possible, by making the length such that the form is very nearly unstable, to cause the bubble to divide the moment the current is made to pass round the electromagnet. With the same means I have not been able to detect any change of form in a spherical bubble. Fig. 15 shows a convenient apparatus for producing, as often as may be desired, a cylindrical bubble of any degree of stability.

The short tube *a* is in connexion with a supply of oxygen which is employed to blow the spherical bubble shown by the dotted circle. According to the position of the screw *d* this bubble will be larger or smaller before it comes in contact with the ring *b*, which is held down by the loose weight *w*. The gas-tap is then immediately turned off, and the ring *b* raised by the action of the weight *c*, until the screw *e* brings the movement to a stop. Thus the length of the cylinder depends on the screw *e*, while its volume is determined by the screw *d*, and so whatever degree of stability is found suitable can be reproduced as often as may be required. The poles of the magnet should be placed at about the level of the line *pp*.

There is one other property of a pair of soap-films resting against one another, but not in contact, to which I have not referred. In a lecture at the Royal Institution a few years ago Lord Raleigh showed that two water-jets if perfectly clean will, if directed so as to meet one another at a small angle, be reflected again and fall as two separate jets, never really coming into contact at all. If the water is not perfectly clean, the experiment will not succeed. He showed that such a pair of mutually reflected jets form a very delicate electroscope, so that if a piece of excited sealing-wax is held even at a considerable distance they instantly coalesce. As the two jets in his experiments and the two bubbles in those which I am about to describe are in each case kept apart by a thin film of air, I expected to find a pair of bubbles attached to two rings in the same way act as a delicate electroscope.

*Exp. 16.*—If a pair of bubbles are blown on rings, which must be insulated from one another, as shown in fig. 16, and the cover of a small electrophorus is raised even at some yards distance, instantly the two bubbles coalesce as seen in fig. 17, but do not burst as they have hitherto been found to



do. Or if the two rings are connected with a key and a single bichromate-cell so that when the key is not pressed the rings are connected together, but when depressed they form the terminals of the cell, then at the moment of making the contact the bubbles unite because the electrostatic attraction between surfaces so very close together is able to squeeze out the air, which mere pressure had hitherto failed to do.

*Exp. 17.*—Bearing in mind how exceedingly delicate this is as a test of difference of potential, the following experiment seems the more decisive. The cover of the electrophorus may be brought so close to the side of the bubbles, shown in fig. 4, as to pull them completely out of shape, and yet the outer film so completely screens the inner from the electrical action, even though the inner one is to all appearance in contact with the outer one, that there is no difference of potential between them, and so the film of air is not destroyed. I do not know any experiment which so clearly shows as this that electrical force is confined to the absolute surface of a conductor, and is not felt at any depth within it however small.

Plateau has mentioned, p. 168, that a hemispherical film blown on a plate will screen a smaller hemisphere blown within it, and also resting on the plate, from electrical disturbance; but in this case the two films are widely separate, and there is not the same delicate test as in the case of two bubbles apparently coincident, which instantly join when the smallest electrical stress exists between them.

*Exp. 18.*—One more experiment, which is a combination of these two, is worth performing. If one of the bubbles of fig. 16 is replaced by the combination shown in fig. 4 while the other remains as before, and if the cover of the electrophorus is raised anywhere in the neighbourhood, immediately the two outer films join and become one, while the inner bubble undamaged and the heavy ring slide down to the bottom of the now enlarged single bubble, and give rise to the form shown in fig. 18.

I am perfectly sensible of the fact that these experiments lie very closely on that ill-defined border-line which separates scientific work from scientific play, but I trust that the beautiful way in which they illustrate the action of certain forces may be sufficient excuse for my showing members of the Physical Society what cannot fail to remind some of them of their nursery days.

The following particulars may be of service to those who wish to repeat any of these experiments.



The solution that I have used is composed of

1	part by weight	oleate of soda.
40	"	distilled water.

These, when solution is complete, are well mixed with one third the volume of glycerine and left for a week to settle in stoppered bottles. The liquid is then syphoned off from the impurities which have risen to the surface and clarified with a few drops of ammonia.

The thick wire rings and frames are made of tinned iron wire  $1\frac{1}{2}$  millim. in diameter, well cleaned with emery cloth.

The thin wire rings may be made of any thin wire, but aluminium about  $\frac{1}{2}$  millim. in diameter does well.

I have found it necessary to make a blowpipe with a trap as shown in fig. 19 to catch condensed moisture, which is apt to cause a failure if it mixes with the bubble. The diameter of the mouth at *a* is 7 millim. For detaching small light bubbles a pipe with a smaller mouth should be used.

When both gas and air are used in any experiment and it is necessary to regulate the proportions very carefully, it is well to have a T-piece attached to the blowpipe, so that either gas or air may be blown or stopped at pleasure.

---

LII. *On the Use of the term "Resistance" in the Description of Physical Phenomena.* By R. H. M. BOSANQUET\*.

THE following observations were suggested by a remark quoted by Prof. S. P. Thompson from Dr. Hopkinson, at the meeting of the Physical Society on January 28, 1888. The remark was to the effect that there is no such thing as magnetic resistance, because the resistance of iron to magnetization is not constant under varying conditions. I wish to bring before you the question, What is the fundamental idea involved in the use of the term "resistance" in the description of physical phenomena?

If we apply force to produce any change, say to extend an extensible body, or compress a compressible one, the body is said to offer resistance to the force. If we have a second body which requires a greater force than the first to produce the same change, we say that the second body offers a greater resistance than the first. Consequently resistance increases with the force employed to produce a given change.

Again, if in the second body the same force produces a greater change than in the first, the resistance of the second

\* Communicated by the Author.

body is said to be less. Consequently resistance diminishes as the change produced increases, and *vice versâ*.

The change may consist in the flow of a fluid, through a tube say, under pressure; then we should say that the resistance offered by the tube to the flow increases as the pressure increases and as the flow diminishes.

Or, again, we may consider a liquid being evaporated under the action of heat. We may say that the resistance to evaporation is the greater the more heat is required and the less liquid evaporated.

We may shortly refer to the two elements in any such change as the cause and the effect. And we shall generalize the conception of resistance in stating that it is the greater the greater the measure of the cause and the less the measure of the effect. But nothing is so far assumed as to the precise law which connects these quantities.

If we now refer to the case of electrical resistance, we have a law suggested by Ohm's law, viz., that the measure of the resistance is the direct ratio of the measures of cause and effect. Or, to take the words "measure of" as understood, resistance is the ratio of cause to effect, or the quotient of cause by effect; or, resistance is proportional to the cause and inversely as the effect.

This is not the least necessary. We might very well, if there were any reason for it in any particular case, adopt as the measure of resistance such a function as the square, or the square root of the above ratio. It would be quite compatible with the general definition deduced above from the meaning of the word. But Ohm's law ties us down to the direct ratio.

If, therefore, we extend the analogy of Ohm's law to other cases, we shall have some such propositions as follows:—

If force be employed to extend an extensible body or to compress a compressible one, the resistance to extension or compression, on the analogy of Ohm's law, is the quotient of the cause, or force employed, by the effect, or amount of change.

If liquid flow through a pipe under a difference of pressure between its ends, the resistance, on the analogy of Ohm's law, is the quotient of the cause, or difference of pressure, by the effect, or rate of flow.

In such cases the precise manner in which cause and effect are measured must be determined by convenience. It does not in the least affect the conception of resistance as the ratio of cause and effect.

Again, if a liquid be evaporated under the action of heat,

the resistance to evaporation, on the analogy of Ohm's law, is the ratio of the cause, or heat expended, to the effect, or amount of liquid evaporated, no matter how these are measured.

We may add a further illustration. Suppose that any substance is heated. Different substances will rise to different temperatures in virtue of their different specific heats. Here the resistance to heating up, on the analogy of Ohm's law, is the ratio of the cause, or heat expended, to the effect, or rise of temperature produced, no matter how these are measured.

In all these cases we have a perfectly intelligible physical conception of resistance, strictly in accordance with the natural meaning of the word in the English language.

But I think we should not apply by way of objection in such cases arguments such as the following:—*i. e.* (1) That the resistance so measured is not of the same dimensions as electrical resistance; or (2) that the resistance so measured is not a constant, but liable to variation depending on the conditions, as well as to complication by other factors.

The ground that underlies the employment which has been made of these two arguments in opposition to the use of the term resistance in connexion with magnetism, is really the assumption that the same word ought to imply identity in the two cases of electricity and magnetism, and not analogy. If it be admitted that analogy is sufficient ground for the use of a word, the objections disappear.

But if used in connexion with two different subjects, it is quite impossible that the meanings of the word can be identical. And that there is no objection in practice to the use of the same word in meanings justified by analogy only may be shown by many illustrations. Take the case of the word Potential. It originated I suppose in connexion with mechanical theory. It has a definite mechanical meaning, and definite dimensions regarded as a mechanical quantity. But we extend the term without difficulty to both the magnetic and electrical analogies; and in each of these cases it has another definite meaning, and other definite dimensions. Yet nobody thinks of objecting to either of these uses of the term potential, because they involve definitions of the dimensions of potential, different both from that used in mechanics and from each other.

Similarly with the word Force. This is a purely mechanical term. But nobody objects to its use in the terms electro-motive force, or magnetizing force because these uses involve



definitions of the dimensions of Force different both from that used in mechanics and from each other.

It is fully understood in these cases that analogy is all that is involved, and nobody supposes that there is any question of identity.

I will now for a moment compare the case of magnetic resistance with a natural case of ordinary resistance, to show that the analogy between the two is extremely close.

Consider a mass of matter in any condition; suppose it enclosed in a vessel and subjected to compression. Then the cause will be the pressure, the effect the compression, and the resistance on the analogy of Ohm's law the ratio of pressure to compression. If we choose to measure the compression by the inverse of the volume, the resistance will be the product of pressure and volume.

If we suppose the matter to be at first gaseous, and the compression to take place with loss of heat according to Boyle and Marriotte's law, the resistance will be so far constant. But as the compression advances, and the liquid and solid states are approached, the resistance will necessarily increase, and when the compression reaches a certain value it will be practically incapable of proceeding further, and the resistance will increase indefinitely with the pressure. The resistance is therefore here a function of the compression, *i. e.* of the effect, just as magnetic resistance is a function of magnetic induction; and, if we omit the initial part of the curves of magnetic resistance of bars, the rest of them is oddly similar to the course of the resistance in the case imagined.

Now the resistance in the case imagined (of the compression of matter) is not constant, but is a function of the effect just as magnetic resistance is. Is this want of constancy an objection to the use of the term resistance as a description of that quality in matter which tends to prevent compression? For if it is a sufficient objection to the term magnetic resistance, it must be a sufficient objection to the term resistance to compression as well, as the analogy between the two cases is so extraordinarily close. It cannot be maintained for a moment that the want of constancy offers any objection.

An objection may be taken possibly that the magnetic resistance has varying values for rising and falling magnetizing force, and is therefore not definitely ascertainable as a function of magnetizing force. Answer: We do not give up trying to find out the true values of things because they are superficially complicated with others. If magnetic



retentiveness does lead to complications, it is our business to disentangle them; and practically the method of reversals of magnetizing current enables us to do this. But it is not maintained, at least by me, that magnetic resistance is properly a function of magnetizing force. It is a function of the effect produced, *i. e.* of the magnetic induction, as is amply proved by the inspection of the numerous curves that have been published in connexion with this question. And it is so just in the same way in which the resistance of matter to compression is a function only of the compression, or of the stage which the condensation has reached.

A word may be usefully said here on the origin of the differential formula used by Lamont to express the so-called magnetic conductivity of a magnet. The ambiguity on which the origin of this formula depends can be well dealt with by the present illustration.

If we consider the compression of matter which has attained the liquid condition, we may speak of the resistance to any further compression at this point, or under these conditions, as being infinite. For the effect produced by the further pressure is nothing. But in the magnetic analogy we always consider a change, the initial condition of which corresponds roughly to the gaseous condition, or rather a pre-gaseous condition, in the material analogy. In either case, if, instead of considering the total change, we consider the state of things at a point, then we must suppose the cause to vary by a small quantity,  $d$  cause, producing a small effect,  $d$  effect; and the ratio of these may be said to be the resistance at the point of the representative curve, or under the given conditions. It is on this mode of statement that Lamont's differential formula is based. The formula is (*Handbuch des Magnetismus*, p. 41),

$$dm = k (M - m) dx;$$

whence by integration,

$$M - m = Ce^{-kx};$$

$x$  = magnetizing force,

$m$  = magnetism,

$M$  = maximum of magnetism.

Here  $dm/dx$  is called the magnetic conductivity; and it is inversely as the *magnetic resistance at a point*. That magnetic conductivity, which is inversely as the magnetic resistance as I and others use the term, is  $m/x$ , *i. e.* has reference to the *total change*. I have shown (*Electrician*, xvi. p. 247) that the assumption  $m/x = k (M - m)$  is that to

which we are thus led, in place of Lamont's formula above given. Prof. S. P. Thompson has shown that this formula, which is the same as that known as Frölich's, corresponds with the facts better than Lamont's.

In what precedes I have, for convenience in quotation, used the ordinary term magnetizing force, and regarded it as a cause. As, however, so-called magnetizing force is identical in dimensions and physical nature with the rest of the magnetic induction which it develops, I use the expression in this way under protest only; for I regard it as inconceivable that cause and effect should be of identical nature, unless the effect reacts again as a cause, so that the smallest original cause drives the effect up to saturation. Without going into detail I may mention an analogy where cause and effect are identical in nature, and the process does therefore necessarily always go to saturation; viz., the case of the multiplication of germs, in a habitat of limited capacity. Here the individuals which formed the original cause multiply; the progeny constitute at once the effect and an increase of the cause. And the smallest original infection of the cause is enough to develop saturation, *i. e.* the highest population that the habitat can maintain. Now in the case of magnetism, magnetizing force and the rest of the magnetic induction are by definition identical in nature. If they are cause and effect, the effect should therefore act again as cause, so as to produce saturation from the smallest force; but this does not occur. I prefer therefore to regard the difference of potential as the cause, as is always done in the case of the electric current.

There is only one further point. It may be maintained that the mention of Ohm's law involves *sub silentio* the assumption that the corresponding law holds true, *i. e.* that the resistance is constant. If this is supposed, it is only necessary to explain that it is not intended. It is clearly legitimate to measure a quantity in the manner suggested by Ohm's law (*i. e.* by the ratio of cause to effect), and speak of the laws or courses of values thus obtained as representing in the different cases the analogues of Ohm's law. These are the laws which occupy the place in the various matters dealt with, which Ohm's law occupies in the subject of the flow of electricity, *i. e.* they express the ratio of cause to effect in the different cases. But the statement that they are the analogues of Ohm's law does not involve the position that they are identical with it, any more than the definitions of Potential or Force in electricity and magnetism are identical with those in mechanics or with each other.

The main points I have dealt with are :—That resistance

may be conceived of quite generally as the ratio of cause to effect.

That the objections to the employment of the term magnetic resistance have been founded on the assumed requirement of identity instead of analogy between the affections of different subjects, and cannot be sustained.

And that the extension of the employment of the term Resistance in the above manner leads to some remarkable analogies which go far to justify independently the employment of the term Magnetic Resistance. And it has been pointed out that the ordinary mode of statement involves inconsistent and impossible ideas as to the relation of cause and effect in the phenomena, whereas the application of the term Magnetic Resistance compels us to precisely ascertain these, and put them in their right places.

*Note.*—An objection has been recently raised by M. Hospitalier\* to my definition of Magnetic Resistance, which comes to this, that in defining the effect I take the induction through unit surface instead of the total induction. This objection is again based on the supposed necessity for conforming identically to Ohm's law in Electricity. The foregoing remarks will have made it clear that I consider this unnecessary, the question being, What is the most convenient measure of the effect? Since the magnetic potential attainable is practically limited, though it increases with the dimension, and the induction through unit of area practically attainable is also limited (by saturation), the ratio of these two quantities lies always within certain limits, though it increases with the dimensions. In fact the magnetic resistance thus estimated is of linear dimension, and may be regarded as of linear scale in plans. If, therefore, this quantity be selected to be tabulated, the discussion of questions of design from a practical point of view is greatly facilitated, and the relations of the quantities involved are more easily followed and more simply expressed.

---

LIII. *Theoretical Essay on the Distribution of Energy in the Spectra of Solids.* By M. WLADIMIR MICHELSON†.

THE recent remarkable publications of Prof. Langley upon the invisible spectrum‡, and especially the promise of a

\* 'Electrician,' xx. p. 164, note.

† Translated from the *Journal de Physique*, t. vi. Oct. 1887. Abstract of a paper recently published in the *Journal de la Société Physico-chimique russe*, vol. xix. No. 4, p. 79 (1887). Communicated by the Author.

‡ S. P. Langley, "Sur les Spectres invisibles" (*Ann. de Chim. et de Phys.* Dec. 1886, pp. 433-506). See also Amer. Journ. Sci. vol. xxxii. Aug. 1886; and Phil. Mag. vol. xxi. pp. 394-409, and vol. xxii. pp. 149-173.

special memoir upon the radiations of solids at different temperatures, induce me to publish at once, at least in abstract, some theoretical considerations upon this subject. I hope to give a more complete discussion of the question when the new data of Prof. Langley's spectro-bolometric researches shall enable me to confront my theory with experiment in a more detailed manner.

1. *Hypothesis and General Law.*—The absolute continuity of the spectra emitted by solids can only be explained by the complete irregularity of the vibrations of their atoms. The discussion of the distribution of radiant energy amongst the simple vibrations of different period is, then, to be undertaken by the calculus of probabilities.

Let us consider a homogeneous isotropic solid of which all the atoms are in identical circumstances, so that, for example, they are not grouped into separate molecules. Each atom has a definite position of equilibrium towards which it is continually driven back by the surrounding atoms, and about which it describes infinitely small oscillations. I express this fact by supposing that each atom moves freely in the interior of a spherical elastic shell of infinitely small radius  $\rho$ , which has the position of equilibrium as its centre. The atom rebounds from the interior surface of this sphere according to the law of impact for perfectly elastic bodies, preserves its absolute velocity during several free paths, and then changes its velocity in consequence of the unsymmetrical action of the surrounding atoms.

Let us endeavour to find upon this hypothesis what will be the most probable trajectories of the atom in the interior of the sphere of displacement. Let us admit that, for the initial position of the atom, all possible distances from the position of equilibrium are equally probable; then the probability that this distance shall lie between the limits  $r$  and  $r + dr$  will be expressed by

$$\frac{dr}{\rho} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Let us take the radius ON (fig. 1) of the sphere passing through the initial position, M, of the atom for polar axis. Let us denote the angle NMP, which the direction of motion of the atom makes with this axis, by  $\phi$ . Let us admit, as in the theory of gases, that all directions are equally probable. Then the probability that  $\phi$  will lie between the limits  $\phi$  and  $\phi + d\phi$  will be

$$\frac{1}{2} \sin \phi \, d\phi.$$

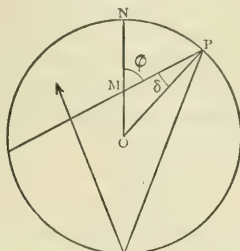
Let us call the angle of incidence MPO,  $\delta$ . This angle is connected with  $\phi$  by the relationship

$$r \sin \phi = \rho \sin \delta.$$



Since the same value  $\delta$  corresponds to the values  $\phi$  and  $\pi - \phi$  of the angle  $\phi$ , the probability that an atom whose initial dis-

Fig. 1.



tance from the centre of the sphere lies between  $r$  and  $r + dr$  shall strike the spherical surface at an angle lying between  $\delta$  and  $\delta + d\delta$ , is given by the expression

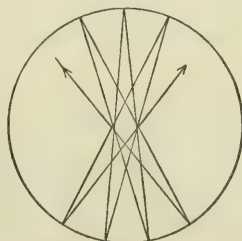
$$\sin \phi d\phi = \frac{\rho^2}{r^2} \frac{\sin \delta \cos \delta d\delta}{\sqrt{1 - \frac{\rho^2}{r^2} \sin^2 \delta}} \dots \dots (2)$$

We obtain the same probability for any atom whatever by multiplying this expression by  $\frac{dr}{\rho}$ , and extending the total to all possible values of  $r$  for a given  $\delta$ ; that is to say, by integrating with reference to  $r$  between the limits  $\rho \sin \delta$  and  $\rho$ ,

$$w = \int_{\rho \sin \delta}^{\rho} \frac{\rho}{r^2} \frac{\sin \delta \cos \delta d\delta}{\sqrt{1 - \frac{\rho^2}{r^2} \sin^2 \delta}} dr = \left( \frac{\pi}{2} - \delta \right) \cos \delta d\delta. \dots (3)$$

It is evident that this probability has a maximum value when  $\delta = 0$ ; that is to say, that motions along diameters of the spheres of displacement are the most probable. It will happen most frequently that, between two neighbouring disturbances, each atom will describe trajectories differing but little from the stellar form (fig. 2).

Fig. 2.



If we suppose that each periodic change in the motion of

the atom produces a wave of the same period in the surrounding æther, and if we consider that the waves produced by the radial motion indicated above will not only be the most frequent, but also the most intense, we must admit, as a first approximation, that it is only these motions which determine the composition of the radiations emitted by our solid.

But the period of vibration  $\tau$  corresponding to this radial motion is evidently connected with the instantaneous velocity  $v$  of the atom by the relationship

$$\tau = \frac{4\rho}{v} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This relationship may be employed for the transformation of the known formula of Maxwell, which expresses the most probable distribution of energy amongst a large number of any material points whatever continually exchanging their velocities by means of forces having a potential\*. If we denote by  $N$  the total number of atoms, by  $m$  the mass of each of them, by  $e$  the base of Napierian logarithms, by  $k = \frac{3}{2mv^2}$  a

constant inversely proportional to the mean *vis viva*  $\frac{mv^2}{2}$  of an atom, Maxwell's law gives the number of atoms whose velocities lie between the limits  $v$  and  $v + dv$ ; this number is

$$r_v = \frac{4N}{\sqrt{\pi}} (km)^{\frac{3}{2}} e^{-kmv^2} v^2 dv \quad . \quad . \quad . \quad . \quad (5)$$

Replacing  $v$  by  $\frac{4\rho}{\tau}$ , according to (4), we obtain

$$r_\tau = \frac{256N}{\sqrt{\pi}} \rho^3 (km)^{\frac{3}{2}} e^{-\frac{16kmp^2}{\tau^2}} \tau^{-4} d\tau \quad . \quad . \quad . \quad (6)$$

This formula gives the number of atoms of which the principal periods of vibration lie between the limits  $\tau$  and  $\tau + d\tau$ . It is the probable law of distribution of the periods of vibration between the atoms of our solid.

In consequence of the law of superposition of vibrations, we may admit that the intensity of a simple radiation of period  $\tau$  ought to be :—

(1) Proportional to the number  $r_\tau$  of atoms of the source of radiation vibrating in the same period.

\* See upon this subject L. Boltzmann, "Ueber die Beziehung zwischen dem zweiten Hauptsatze der mechanischen Wärmetheorie und der Wahrscheinlichkeitsrechnung &c." (*Sitzungsber. der Wiener Akad. der Wiss.* Bd. lxxvi. pp. 373-435, Oct. 1887); and also Watson, 'A Treatise on the Kinetic Theory of Gases' (Oxford, 1876), p. 12, &c.

(2) Proportional to a function of the energy of these same atoms. In consequence of the relationship (4) and of the high value of  $\frac{1}{\tau}$  for all the radiations which we shall have to consider, this function may be reduced to a power of  $\frac{1}{\tau^2}$ .

(3) Finally, in the direct ratio of an unknown function of the absolute temperature of the body. This function constitutes a factor which ought to represent the mean reinforcement or weakening produced in each primary wave by the whole of the resultant vibrations\* and by absorption in the radiating body itself. We will denote this function by  $f(\theta)$ .

Thus, then, denoting positive constants by A and  $p$ , we put the intensity of the simple ætherial undulation of the period  $\tau$ ,

$$I_{\tau} = A v_{\tau} \left( \frac{1}{\tau^2} \right)^p f(\theta). \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Considering, as usual, the absolute temperature  $\theta$  as proportional to the mean *vis viva* of an atom, we may replace the constant  $k$  in formula (6) by  $\frac{M}{\theta}$ , where M is independent of  $\theta$ .

Let us then introduce this expression (6) in equation (7). Then putting, for the sake of brevity,

$$A \frac{256N}{\sqrt{\pi}} \rho^3 (Mm)^{\frac{3}{2}} = B, \quad 16\rho^2 Mm = c: \quad . \quad . \quad . \quad (8)$$

we have

$$I_{\tau} = B \theta^{-\frac{3}{2}} f(\theta) e^{-\frac{c}{\theta \tau^2} \tau^{-(2p+4)}} d\tau. \quad . \quad . \quad . \quad (9)$$

Replacing here the variable  $\tau$  by the variable  $\lambda = V\tau$ , where  $\lambda$  is the length of the æthereal wave, and V the velocity of propagation of light. Thus denoting constant coefficients suitably modified by B and  $c$ , we shall have

$$I_{\lambda} d\lambda = B \theta^{-\frac{3}{2}} f(\theta) e^{-\frac{c}{\theta \lambda^2} \lambda^{-(2p+4)}} d\lambda. \quad . \quad . \quad . \quad . \quad (10)$$

This formula gives the intensity of a simple radiation of wave-length  $\lambda$  as a function of this length and of the absolute temperature of the source.

\* According to the analysis of Helmholtz, resultant waves must be produced in all cases where the square of the elongation is not to be neglected. They explain, according to this author, the phenomena of sound of Sorges and Tartini (combination tones). In the case which we are considering they ought to form especially upon the surface of the radiating body, the forces which retain the atoms in their positions of equilibrium not being symmetrical in all directions.

It is towards a law of this sort that the distribution of radiant energy in each spectrum must tend as its continuity becomes more and more perfect, that is to say, as the elective absorptions along the path of the rays diminish. To obtain a more complete expression of the law it would still be necessary to determine the value of  $p$  and the form of  $f(\theta)$ ; but our formula is capable of giving some interesting results without our even having to make such a specialization.

It is evident that, under this general form, our law embraces as particular cases all the empirical laws of emission proposed hitherto, such as those of Newton, of Dulong and Petit, and of Stefan.

If we were to attribute to  $\theta$  a constant value, and if we were to take  $\lambda$  as abscissa, and  $I_\lambda$  as ordinate, equation (10) would be the equation of the curve of energy in the normal continuous spectrum of a solid source at the temperature  $\theta$ . These are the curves which M. Crova has called "isothermic curves."\* In order to study the general properties of these curves, we will suppose  $\theta$  constant and take the derivative of the expression  $I_\lambda$ .

2. *Limits of the Spectrum.*—It is easy to see, from formula (10) and its derived formula, that if  $f(\theta)$  and  $p$  have definite values, we shall have for  $\lambda=0$  and  $\lambda=\infty$ ,

$$I_\lambda=0 \quad \text{and} \quad \frac{dI_\lambda}{d\lambda}=0.$$

This signifies that all the curves of energy represented by equation (10) are tangents to the axis of  $\lambda$  at the origin of coordinates, and that they have this axis as an asymptote. At the two extremities of the spectrum the radiant energy diminishes to zero; but whilst towards the violet it disappears almost suddenly because of the rapid diminution of the factor  $e^{-\frac{c}{\theta\lambda^2}}$ , the less refrangible energy extends indefinitely towards the side of increasing  $\lambda$ . This fact has been recently observed by Prof. Langley†.

3. *Maximum Intensity.*—Each of the curves represented by equation (10) presents only a single maximum defined by the condition

$$\lambda_{\max} = \sqrt{\frac{c}{p+2}} \frac{1}{\sqrt{\theta}}. \quad \dots \dots (11)$$

We see that the position of maximum intensity in our spectra

\* A. Crova, "Etude des Radiations," &c. (*Ann. de Chim. et de Phys.* [5] t. xix. p. 472).

† S. P. Langley, "Sur les Spectres invisibles" (*Ann. de Chim. et de Phys.* Dec. 1886).



depends upon temperature, and formula (11) gives the law of this dependence :—*Whatever the law of emission may be, the wave-length corresponding to the maximum energy is inversely proportional to the square root of the absolute temperature of the source.*

The only experimental measurements, so far as I know, which have been published hitherto upon this subject are contained in a recent diagram by Prof. Langley\*. In order to compare these data with the relationship  $\theta\lambda_{\max}^2 = \text{const.}$  equivalent to (11), I have deduced from this diagram, by a graphical process, the following values of  $\lambda_{\max}$ :—

Temperatures.			
$^{\circ}\text{C.}$	$^{\circ}\text{F.}$	$\lambda_{\max}$	$\theta\lambda_{\max}^2$
178	451	4.90	10828
330	603	4.05	9891
525	798	3.63	10515
815	1088	3.27	11634

Although the constancy of  $\theta\lambda_{\max}^2$  is far from being perfect, yet in view of the manner in which these four values have been obtained, and the uncertainty attaching to the determination of elevated temperatures, we may say that these observations appear to confirm our theory.

4. *Measurement of Temperatures.*—If we suppose that this law were confirmed by more numerous observations, it would then furnish us with a very simple method of measuring the high temperatures of radiating bodies†. This method would be superior to the spectro-photometric method elaborated by M. Crova, inasmuch as it is not dependent upon the choice of the tints compared, and would give, not “optical degrees,” but absolute temperatures. Knowing the constant of equation (11), it would suffice, in order to measure the temperature of a radiating body, to determine the wave-length corresponding to the maximum intensity of its normal spectrum.

If we take account, as much as possible, of the absorptions suffered by the solar rays, and if we admit that the great density and enormous mass of the sun ought to give to its primitive spectrum all the qualities of continuous spectra, we may apply this method to calculate the temperature of the sun. According to the observations of Prof. Langley‡, the maximum

\* *Loc. cit.* p. 462.

† The idea of a similar method, but without any definite law, has been propounded by M. Crova, *loc. cit.* p. 479.

‡ S. P. Langley, ‘Researches on Solar Heat,’ a report of the Mount Whitney Expedition, p. 144, pl. xv. curve iii. (Washington, 1884).

intensity of the solar spectrum observed beyond our atmosphere would be situated about  $\lambda = 0^{\mu}5$ . Hence if, according to the preceding table, we take for  $\theta\lambda_{\max}^2$  the round number 10,000, we obtain about  $40,000^{\circ}$  for the absolute temperature of the sun. It should be remarked that this method only supposes an analogy in the distribution of energy in the primitive spectrum of the sun and in that of lampblack; but the total emissive power may be very different for these two bodies.

5. *Total Energy of Radiation*.—By calculating the area of each curve represented by the equation (10), I obtain the total energy of radiation of the spectrum

$$E = \int_0^{\infty} I_{\lambda} d\lambda = \frac{1}{2} B e^{-(p+\frac{3}{2})} \Gamma(p+\frac{3}{2}) f(\theta) \theta^p, \quad . \quad . \quad (12)$$

where  $\Gamma$  denotes the Eulerian integral of the second order.

Comparing this formula with that obtained by eliminating  $\lambda$  between the two equations (10) and (11), that is to say, with the maximum intensity of the spectrum,

$$I_{\max} = B \left( \frac{p+2}{ec} \right)^{p+2} f(\theta) \theta^{p+\frac{1}{2}}. \quad . \quad . \quad . \quad (13)$$

Making abstraction of constant coefficients, expressions (12) and (13) only differ because the second contains another factor  $\sqrt{\theta}$ . Hence the maximum intensity increases with the temperature more rapidly than the total energy of radiation, and their ratio

$$\frac{I_{\max}}{E} = \frac{2e^{-\frac{1}{2}}}{\Gamma(p+\frac{3}{2})} \left( \frac{p+2}{e} \right)^{p+2} \sqrt{\theta}, \quad . \quad . \quad . \quad (14)$$

increases in the direct ratio of the square root of the absolute temperature of the source.

Multiplying together equations (11) and (14), we obtain

$$\frac{I_{\max} \lambda_{\max}}{E} = \frac{2e^{-(p+2)}}{\Gamma(p+\frac{3}{2})} (p+2)^{p+\frac{3}{2}} = \text{const.} \quad . \quad . \quad (15)$$

This interesting relationship shows that *the total radiant energy emitted by a solid bears a constant ratio to the product of the maximum energy of the normal spectrum by the corresponding wave-length*, or, differently expressed, that the area of each curve of energy bears a direct ratio to the area of the rectangle having for sides the coordinates of the summits of this curve (see further on, fig. 3). This constant ratio depends only upon the value of  $p$ , which probably is the same for all bodies; for in our equations it is this value which characterizes the

mode of action between the ponderable atoms and the surrounding æther.

This relation, which does not require a knowledge of the absolute temperature, is better fitted for experimental verification than the preceding relations. For the moment we do not possess direct data for such a verification. It is only in a very imperfect manner that I have been able, by measuring the ordinates and areas of the curves given by Prof. Langley, to obtain the following values of  $I_{\max}$  and  $E$  :—

$\theta$ .	$\lambda_{\max}.$ $\mu$	$I_{\max}.$	$E.$	$\frac{I_{\max} \lambda_{\max}}{E}.$
451 . . .	4.90	5.2	182	0.140
603 . . .	4.05	21.5	532	0.163
798 . . .	3.63	39.7	1074	0.134
1088 . . .	3.27	64.9	1730	0.123

Bearing in mind the imperfection of the method of verification, the numbers of the last column, which according to our theory should be equal, do not seem unfavourable to it.

6. *Tracing the Curves.*—To be able to trace the curve of energy it is necessary to give special values to  $p$  and  $f(\theta)$ . If, for example, we admit with M. Stefan\* that the radiant energy emitted by a body bears a direct ratio to the fourth power of its absolute temperature, we shall be able to put

$$p=1, \quad f(\theta)=R\theta^3;$$

from which equation (10) will take the form

$$I_{\lambda}=B_1\theta^3e^{-\frac{c}{\theta\lambda^2}}\lambda^{-6}. \quad . \quad . \quad . \quad . \quad . \quad (16)$$

Having determined the two constants  $B_1$  and  $c$  of this equation from a diagram of the normal invisible spectrum of lampblack at  $178^{\circ}$  C., given by Prof. Langley, I have calculated and traced the theoretical curves for this temperature and for  $327^{\circ}$  C. =  $600^{\circ}$  abs. (fig. 3). We see that these curves have all the qualities that Prof. Langley attributes to the curves determined experimentally by him ; that is to say :—

(1) The radiant energy represented by the ordinates diminishes on both sides, yet extends indefinitely towards the side of the less refrangible rays.

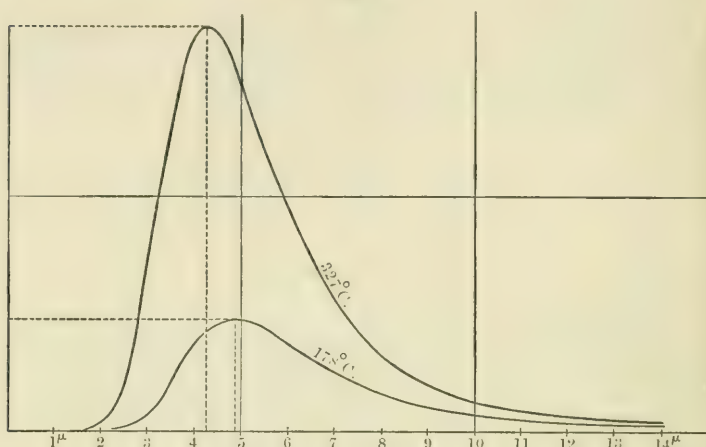
(2) When the temperature increases all the ordinates increase, but in unequal proportion. Those which represent the energy of the more refrangible waves increase always more rapidly than those which correspond to the longer undulations.

(3) Hence the fact, independently observed, that the maxi-

\* Stefan, *Sitzber. Wien. Akad.* vol. lxxix. p. 391 (1879).

num ordinate is progressively displaced toward the more refrangible region. We have indicated above the probable law of this displacement.

Fig. 3.



(4) The theoretical curves of the normal spectrum, like Prof. Langley's prismatic curves, are not symmetrical; the largest area is situated on the right-hand side of the maximum ordinate, that is, on the side of the less refrangible rays.

(5) In accordance with this, the fall of each curve is more rapid on the side of the shorter undulations. For the two cases of which we have given diagrams, all sensible heat disappears before the curves reach the limit of the visible spectrum: there is only dark heat.

7. *Solar Curve.*—I have also endeavoured to compare my theoretical curves, as to general form, with the curve of solar energy beyond the atmosphere. In fig. 4 the separate points correspond to the numbers given by Prof. Langley in No. 9 of the table 120 of his work, 'Researches on Solar Heat, &c.' The dotted curve has been traced from curve 3 of plate 15 of the same work, and the continuous curve gives the theoretical curve represented by the equation

$$I_{\lambda} = A e^{-\frac{M}{\lambda^2} \lambda^{-6}}, \quad . \quad . \quad . \quad . \quad . \quad (17)$$

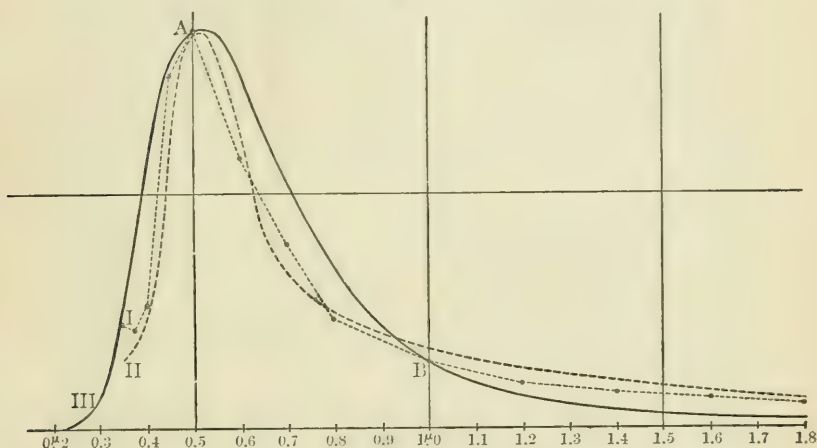
and passing through the observed points A and B.

To take account of the difference between theory and observation (a greater difference in this case than in the preceding one), we must remember that not only equation (17), but also



the more general formula (10), can only be considered as a first approximation ; that these equations have been deduced for solid bodies, and not for such bodies as the sun ; and, lastly, that in our atmosphere, as well as in the colder regions of the gas surrounding the sun there must occur the pheno-

Fig. 4.



menon of degradation of the radiant energy. In consequence of this effect, which has not been eliminated by Prof. Langley, a portion of the more refrangible radiations would be transformed into longer undulations, which would tend to modify the spectral diagram exactly in the sense indicated by observation.

Recognizing the provisional character of the ideas suggested in this paper, I hope that it will be possible to give them a greater development and precision when once we are in possession of more complete experimental data to guide us in this study. We should then be able to express by one sufficiently exact formula the law of distribution of radiant energy as a function of the wave-length and the temperature of the source. Such a formula would be of great service in all questions arising in the study of spectra.

LIV. *Notices respecting New Books.*

*The Testing of Materials of Construction. A Text-book for the Engineering Laboratory and a Collection of the Results of Experiment.* By WILLIAM CAWTHORNE UNWIN, F.R.S., M. Inst. C.E., Professor of Engineering at the Central Institution of the City and Guilds of London Institute, formerly Professor of Hydraulic and Mechanical Engineering at the Royal Indian Engineering College. (London: Longmans, Green, and Co., 1888.)

The present work is a treatise on the strength of materials used in construction, considered in connexion with the instruments and methods by which the properties of materials are investigated experimentally.

The treatise consists of three parts. In the first, the mechanical properties of materials are explained—that is, their elasticity and plasticity, and the relations between stress and strain. In the second, the apparatus used in the engineering laboratory is described. Lastly, the third part contains a collection of the most complete and trustworthy results of testing, of all the ordinary materials of construction.

After an introduction, which the reader will do well to reperuse when he has gone through the whole work, the author devotes three chapters to an explanation of the mechanical properties of materials. His explanations here, and indeed throughout the book, are lucid and can be readily followed even by those who may not be in possession of very advanced mathematical knowledge. Some of the deductions, however, which the author makes from the experiments which he quotes do not seem to us to be quite justifiable. We find for instance on page 89 the statement, “Some remarkable experiments of Colonel Maitland at Woolwich show that, contrary to common prejudice, the ultimate elongation is increased by very rapid loading.” We do not think that Colonel Maitland’s experiments, which were made on bars of unhardened steel only two inches in length, settle the question, especially as we learn on page 291 that “M. Barba has stated that in rapid testing, the resistance is somewhat greater and the elongation less than in slow testing.” We rather incline to the opinion that when the length of the piece tested is large compared with the sectional area, slow testing, if not too slow, will often produce a greater ultimate elongation than very rapid testing. In the case of iron and steel very slow testing will always produce less elongation than rapid testing. Again, though M. Tresca observed with lead and other plastic metals that large plastic deformation was unaccompanied by any sensible change in the density, the reader should not be allowed to infer, as he probably would, that there is no sensible change in density accompanying large plastic deformations of such metals as copper and iron. The density of copper may be

increased nearly 5 per cent. by compression between the dies used in coining, and 1 per cent. by simple longitudinal extension. Such changes of density are not great, but it would have been better, we think, to have given the reader some idea of their extent.

We would pause here for a moment to observe that though Professor Unwin has wisely not confined himself to consulting only the memoirs of engineers, but has also paid some little attention to the experiments which physicists have made on elasticity, he might have still further profited by the latter. We venture to say that a closer inspection of such memoirs as those of Wertheim (*Annales de Chimie*, vol. xii. 1844), G. Wiedemann (Wiedemann's *Annalen*, 1879, vol. vi. p. 485, and elsewhere), and of Herbert Tomlinson (*Phil. Trans.* 1883, 1886), would have been not without profit. The experiments of Wiedemann are especially valuable and throw very considerable light on the effect of repeated stress whether of bending, torsion, or extension. Tomlinson has also shown that the internal friction of metals does *not* depend upon the velocity with which deformation is taking place, whereas we are told on page 47 that it does.

"The author has had opportunity of examining nearly every form of testing machine, and of using very nearly all the subsidiary measuring and other apparatus here described." As might be expected, therefore, the next four chapters, which deal with the apparatus used in the engineering laboratory, are good, and indeed could not well be better. It is impossible to read the description of the 450-ton Emery Testing Machine at Watertown Arsenal, U.S.A., without being strongly impressed. "This machine is probably the largest and most accurate testing machine in the world. Before acceptance by the Board a link of hard iron, 5 inches in diameter, was placed in the machine, and slowly strained in tension till it broke at 722,000 lbs. Without any readjustment a horse-hair was then fixed in the machine and broken at an indicated stress of 1 lb."

The last eight chapters are occupied with the results of testing. Out of an enormous mass of data which has accumulated during the last forty years, a very judicious selection has been made. The tables of results which are given are all the more valuable in that they have all been reduced to common units and are consequently most easily understood and compared.

We are sorry to learn from Professor Unwin that in most cases Young's Modulus is measured by the ratio of longitudinal stress and strain, when the last includes not only temporary but also permanent and subpermanent strain. It is no wonder that even the careful observations of Professor Bauschinger (p. 250) show variations in the modulus which actually amount to 10 per cent., and this too with instruments which would enable him to determine the modulus within one-half per cent. According to Young, his modulus of elasticity is the amount of end-pull or end-thrust required to produce any *infinitesimal* elongation or contraction. Now provided a bar be loaded and unloaded a number of times with

any fairly small temporary load (say not exceeding one tenth the breaking-load) until the elongation which is produced on putting on the load is equal to the contraction which ensues on taking it off, and the ratio of the temporary stress to the temporary strain then produced be taken as measuring Young's modulus, we obtain a result which is practically the same as when the deformation is infinitesimal, one which is independent of the amount of permanent load and, provided in the case of *iron and steel* which have been permanently deformed rest be allowed, one which is independent of any previous permanent extension of the metal.

There is a slight slip in putting  $\frac{1}{m}$  for Poisson's ratio on p. 20 and  $m$  for the same ratio on p. 253, but on the whole the book has evidently been written with great care; and we heartily congratulate Professor Unwin on having produced a work which is eminently adapted for the purpose for which it is intended, namely, to be a text-book for the engineering laboratory.

The book is copiously illustrated, and is in every respect well worth its price, 21s.

#### LV. *Proceedings of Learned Societies.*

##### GEOLOGICAL SOCIETY.

[Continued from p. 322.]

March 14, 1888.—W. T. Blanford, LL.D., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On the Gneissic Rocks off the Lizard." By Howard Fox, Esq., F.G.S.; with Notes on Specimens by J. J. H. Teall, Esq., F.G.S.

The rocks may be classed under three heads:—(i.) the coarse gneisses or Mên Hyr type, (ii.) the light-banded granulitic gneisses or Wiltshire type, and (iii.) the transition micaceous rocks of "Labham Reefs," type intermediate between (ii.) and the mainland schists.

The first are seen in Mulvin, Taylor's Rock, Man-of-war Rocks, the Stags, Men Par, Clidgas, Mên Hyr, and Vasilier; the second in Sanspareil, the Quadrant, and adjoining reefs, Labham Rocks, &c.; and the third in the Labham reefs.

The inclination of the divisional planes appeared conformable with that of the rocks of the mainland.

The gneisses and granulites of several of the islands are traversed by numerous dykes of porphyritic basic rock, seen in Taylor's Rock, Man-of-war Rocks, Sanspareil, Quadrant Rock and Shoals, and Clidgas. These dykes have been disturbed by movements subsequent to their intrusion. They sometimes strike across the foliation-planes of the gneiss and send veins into the latter rock; at other



times the strike is parallel to that of the foliation-planes; the two modes of occurrence are occasionally observable in different portions of the course of the same dyke, *e. g.* in one traversing that part of the Man-of-war group known as the Spire. This dyke is also noticeable from the fact that it appears to be traversed by veins of gneiss.

The dykes vary in width from 18 inches to several feet.

In his notes on the specimens Mr. Teall says that the rocks may be arranged in four groups:—

1, principally occurring in the outer islands, are of the Mên Hyr type, consisting of felspar, quartz, dark mica, and hornblende; the quartz and felspar sometimes exhibit relations characteristic of igneous rocks, at other times they form a fine-grained granulitic aggregate, the latter being probably the result of dynamic metamorphism. This granulation is carried to a greater extent in some of the islands, as in Taylor's Rocks. The rocks possess the mineralogical composition of quartz diorite, and may be termed tonalite-gneisses; they may originally have been eruptive tonalites.

2, occurring chiefly in the inner islands, are of the nature of granulitic gneisses and granulites, confining the latter term to rocks in which the quartz and felspar are present wholly in the form of a micro-crystalline mosaic of fairly uniform grain. In some of these foliation is not well marked. Such rocks occur in "Wiltshire," &c.

3. Rocks showing a passage from the granulitic rocks to the mica-schists of the mainland, as the brown schistose rocks of Labham Reefs. The Enoch rock, a coarse quartzless hornblende schist, also has affinities with the mainland schists.

4. Dykes traversing the gneisses, consisting of porphyritic felspars lying in a ground-mass of hornblende and granulitic felspar. The hornblende is probably secondary after augite, and the rocks epidiorites. These dykes have been affected by deformation, and sometimes pass into actinolite schists near the junction with the gneissose rocks.

In conclusion, the period of dynamic metamorphism, of which the most striking results are seen in the schists of the south-western portion of the Lizard peninsula, was posterior to the formation of the basic dykes. There is no evidence of igneous action in this district since the period of metamorphism.

2. "The Monian System." By the Rev. J. F. Blake, M.A., F.G.S.

The object of the author was to show that the whole of the rocks which, under various names, had been described as Pre-Cambrian in Anglesey constitute a single well-characterized system, of which the various divisions hitherto described are integral and inseparable parts.

The evidence of these rocks being Pre-Cambrian was first discussed, and it was shown that the greater part of it went no further than to prove them Pre-Ordovician, the basal conglomerates being associated with rocks of Arenig age, though from the occurrence of these

conglomerates on Holyhead Island it was inferred that the previous denudation had been great. The rocks of the eastern district, however, are proved to be Pre-Cambrian from the basal Cambrian rocks of Bangor type lying on them unconformably near Beaumaris and near Redwharf Bay. The rocks described are found in six distinct districts in Anglesey.

1. *The Western District.*—The lowest rocks are the great quartzites of Holyhead, which pass at Porth-yr-ogof and inland into chloritic schists, which are foliated in planes of lamination, and thus is produced a tough rock which will not cleave or break, but bends into minute contortions. Towards the east this becomes finer in grain, and may be distinguished as chloritoid schist. On the side of the Straits near the valley it may be seen passing into purple slate. Further north the rocks are confused, especially at Porth-y-defaid, but there is no well-defined fault. The material becomes irregular and forms rocks described as “marbled slate,” “lenticular pelites,” and soft tuffs. Amongst such are found two special features, viz. masses of quartz in the form of knobs, and lenticular patches of limestone. These it is suggested were produced by the agency of springs rising through and into the ashy rocks. They are specially characteristic of this part of the series. The granite of Pen-bryn-yr-Eglwys is intrusive, and its junction may be seen in several places, the surrounding rocks developing mica. It is here therefore the youngest rock.

On the south-west side of a fault in Holyhead Island and the neighbouring mainland occurs a distinct group of rocks, continuous upwards from the chloritoid schists, and equivalent to the volcanic facies of the north. These are called the South Stack series. They are characteristically bedded, thrown into large folds, sporadically cleaved, and possess cleavage-foliation. They contain great beds of quartzite, and others of light dusty material. When not cleaved they are almost entirely unaltered.

The spot near Tywyn, supposed to show fragments of granite contained in a rock of the upper series, and hence the conformity of the two, shows only an intrusive diabase which has caught up granitic fragments.

2. *The Central District.*—This is divided into two parts by a fault. That on the east consists of grey gneiss, considered to be the lowest rock of the whole series, with the quartzite at Bodofon as an episode, followed after a fault by chloritoid schists, so intimately connected with the overlying volcanic facies as to be inseparable. The principal features of the latter are the agglomerates of Llangefni, the quartz knobs, and the more or less bedded sporadic limestones. The portion on the west consists of ashes and fine hálleflintas, together with gneissose rocks of no great similarity to the grey gneiss. These have been so interfered with by intrusive rocks that it is difficult to ascertain their true original character. These intrusions consist of (1) Diorite, often foliated, with the folia contorted, and affording by its brecciation some portion of the surrounding rocks. (2) Granite, seen everywhere to be either intrusive, as at Porth-y-

dee, Ceryg-defaid, Craig-y-allor, and Maen-wyn, or absorptive, as near Llyn-faelog. This granite is of various kinds, often passing into a felsite.

3. *The District west of Trueth Dulus* shows granite intrusive into grey gneiss, and also passing into a felsite; it is correlated with the western half of the central district, of which it appears to be a continuation.

4. *The Eastern District.*—The lowest portion here is the grey gneiss, which is very compact towards the west, especially near the igneous rocks, but becomes more micaceous and chloritic towards the east, passing through chloritoid rocks into others of the volcanic facies, with the usual quartz knobs and sporadic limestones, but here, on the whole, more slaty. The complete unity of the whole system is here well seen. The most remarkable feature is the intrusive foliated diorites, which are coarse and non-foliated near Holland Arms, but become finer towards the east, where also glaucophane takes the place of hornblende. They are seen intruding into and contorting the grey gneiss in the Llangaffo cutting.

At the southern end at Careg-gwladys is a remarkable volcanic group, with a spherulitic diabase breaking into and surrounding the baked blocks of calcareous slate. There are associated great masses of mixed agglomerate, and terminated masses of limestone and quartzite filled with brecciated fragments.

5. *The Northern District.*—This commences in the south with the chloritic schists, but soon becomes slaty, and such rocks with grits occupy the greater part of the area. But towards the north we reach the volcanic facies, characterized as usual by ashes, agglomerates, quartz knobs, in one case, near Bull Bay, seen to cross the bedding, and sporadic limestones at Llanbadrig, showing oolite within oolite, and suggesting its origin by a petrifying spring. Above the quartz is found a great conglomerate, apparently derived from it, and immediately above this conglomerate occur the fossils discovered by Prof. Hughes, and no line of separation can be discovered between them and the rest of the rocks in the district. These fossils have been referred to Bala species, and there are three alternatives to choose: either (1) they are not Bala fossils, but are characteristic of the Pre-Cambrian rocks; or (2) they are Bala fossils, and the dividing line has as yet been missed; or (3) there is no dividing line, and the whole series is of Bala age. Against the latter is their similarity to the rocks of the eastern district definitely overlain by Cambrian; and against both the two latter is the fact that the series is unconformably overlain in the neighbourhood by other conglomerates succeeded by black shales in which Llandeilo Graptolites have been recorded.

6. *The District north-east of Parys mountain* is a volcanic complex, in which granitic and felsitic rocks with others of a more basic character are inextricably mixed with the débris of the same materials, and both are altered so as to be, in most places, inseparable. This is connected on the N.W. side with grey gneiss.

In the *Llyn* the rocks belong to the volcanic facies, in which



great masses of quartz-felsite, foliated at the edges, are intruded. Here also are found the quartz knobs and sporadic limestones as well as diabase-flows. At Mynydd ystum is found an isolated patch of grey gneiss.

The area between Bangor and Cuernavon has lately been shown to contain some felsite-flows, and also granites, apparently intrusive into ashes, which may belong to the volcanic facies.

At Howth, near Dublin, the rocks have all the characters of the South-Stack series, to which they may be correlated; and these are followed upwards by the well-known Bray-Head rocks, which differ from them in character, but whose fossils are not of Cambrian species.

The succession thus shown in the various districts consists of the following in ascending order. The grey gneiss, becoming more quartzose, micaceous, or chloritic in parts, and so representing the quartzite and the chloritic schists of other districts; changing through chloritoid schists into two facies, viz., (1) the slaty, represented best in the northern district, and also as the South-Stack series; and (2) the volcanic facies. No further deposits are recognized in the areas of the volcanic facies, but in the slaty area of Howth the Bray-Head rocks succeed.

To the whole system of rocks the name of MONIAN is applied, as derived from Mona, or the Isle of Anglesey, and the several parts are distinguished as the *Holyhead group*, or Lower Monian, the *St. David's group* and the equivalent of the *South-Stack Series*, or Middle Monian, and the *Bray-Head group*, or Upper Monian.

The "Pebidian" represents the St. David's group, and but for its termination, which indicates a system, might be used as an alternative. The "Dimetian and Arvonian" are intrusive granites, or felsitic flows associated with the same group.

The Monian system, though much metamorphosed in its lowest parts, is not considered Archæan, but as a lower sedimentary system than the Cambrian, and hence the lowest system of our ordinary stratified rocks.

March 28.—W. T. Blanford, LL.D., F.R.S., President,  
in the Chair.

The following communications were read:—

1. "On some Eroded Agate Pebbles from the Soudan." By Prof. V. Ball, M.A., F.R.S., F.G.S.

The majority of the pebbles in a collection made by Surgeon-Major Greene in the Soudan, and presented by him to the Science and Art Museum in Dublin, are of very similar character to the agate and jasper pebbles derived from the basalts of India. It may be concluded inferentially that they came originally from a region in which basaltic rocks occur to a considerable extent. A certain number of them are eroded in a manner unlike anything noticed in India, though it is probable that similar eroded pebbles will eventually be found there.



Throughout India, wherever there is deficient subsoil-drainage or excessive evaporation and limited rainfall, salts are apparent either in supersaturated subsoil-solutions or as crystallizations in the soil. They are most abundant in basaltic regions, and in a lake occupying a hollow in the basalt in Berar carbonate of soda is deposited in abundance from the water, which becomes supersaturated during the summer.

The author commented on the efficacy of such a liquid as a solvent of silica, and noticed the selective action of the agent which had affected the Soudan pebbles and had corroded some layers more than others; he suggested that while this might be to some extent due to differences in composition, it was more probably owing to differences of nodular constitution. He considered it unnecessary to refer to the action of humic acid, because, while the salt to which the solvent action is attributed would be capable of doing such work, and would be probably abundant in the region referred to, we could not expect any great amount of humic acid in the same area.

2. "On the Probable Mode of Transport of the Fragments of Granite and other Rocks which are found imbedded in the Carboniferous Limestone of the neighbourhood of Dublin." By Prof. V. Ball, Esq., M.A., F.R.S., F.G.S.

Angular fragments of granite and of schist, quartzite, and vein-quartz, such as might have been derived from the metamorphosed rocks which rest on the granite near Dublin, have been discovered in beds of Carboniferous Limestone, which often contain fragments of fossils, especially *Encrinites*. They have been previously noticed by Professor Haughton, Mr. H. B. S. Montgomery, Prof. Jukes, and Mr. Croll. While Prof. Jukes refers their transportation to the agency of land-plants, Mr. Croll quotes their occurrence in support of his argument as to the existence of glacial conditions during the Carboniferous period.

The author observed that the specimens exhibited none of the indications of the existence of glacial conditions, whether we regard the characters of the boulders or the nature of the rock in which they are imbedded, which contains no such silt as that occurring in the boulder-bed of the Talchir formation. Whilst rejecting the view that they were transported by ice, he pointed out that they need not necessarily have been carried by land-plants, but that they might have been torn from the sea-floor by marine algæ, some of which may have had a more buoyant character than those of modern seas. He cited the case of a sandy beach in the neighbourhood of Youghal, which is strewn with limestone fragments, which had been conveyed by sea-weeds thrown up after storms from submarine banks.

It was suggested that the occurrence of natural fissures in the rocks and cracks produced by concussions from large masses hurled about by the waves might sufficiently explain how the fragments could be freed from the main mass of the reefs under the stress of the waves.

3. "The Upper Eocene, comprising the Barton and Upper Bagshot Formations." By J. Starkie Gardner, Esq., F.G.S., and Henry Keeping, Esq.; with an Appendix by H. W. Monckton, Esq., F.G.S.

The familiar Upper Eocene having been transferred to the Oligocene, the remaining uppermost division of the Eocene bears the title Middle. Unless the considerable literature relating to the Brackleshams, the Calcaire Grossier, and the Nummulitic, is to be rendered obsolete, their classification as Middle Eocene must be preserved, and a modified Upper Eocene constructed out of the Barton series. The authors' proposal is that the following should be adopted:—

	London Area.	Hampshire Area.
Upper Eocene {	—	Upper Barton.
	—	Middle    "
	Upper Bagshot Sands.	Lower     "

The base of the formation is not sharply defined, but it coincides with the final disappearance of several subtropical Mollusca, and almost with the extinction of Nummulites in our area. The upper limit is drawn at the base of the Lower Headon, where the brackish fauna gives place to one of fresh water.

The conditions of deposition were examined at some length, and evidence in support of the estuarine origin of the formation was adduced. The section in Christchurch Bay was described first, and the thickness and characteristics of each subdivision given, the total reaching 170 to 180 feet. It commences with 45 feet of whitish sand, and in ascending order, a pebble-bed, 11 feet of greenish clay, and a band of imperfect ironstone underlying the zone of *Nummulites elegans*. Then 20 feet of stiff drab clay, 13 feet of drab clay with sand-drifts, and 12 feet of the same, known as the Highcliff Sands. The Lower Barton terminates with the *Pholadomya*-bed. The fauna of this division comprises many Bracklesham species, which range no farther up, and a large number of peculiar species. The most convenient base-line for the Middle Barton is the lowest of several bands of *Septaria*, which distinguish the 50 feet of drab clays which are comprised in it, and it terminates in a very remarkable formation known as the shell-bed, which though only a foot or two thick at Highcliff, thickens to about 15 feet to the east, and to 22 feet in the new Christchurch cutting. The finest Barton fossils are obtained from the Middle division; but though so many splendid species characterize it, few are absolutely confined to it. The upward range of a further number of Bracklesham species ceases at the shell-bed. The Upper Barton includes the *Chama*-bed, the Beeton Bunny and the Long Mead End beds.

The Beeton Bunny beds, 52 ft. thick, are sand in the lower half and sandy clay above—*Oliva Branderi* is the characteristic fossil, and a large number of bivalves and brackish Headon Gasteropods come in. Opinions have differed considerably as to whether these beds should be included in the Bartons. The series closes with the Long-Mead-End Sands, 20 ft. thick, with similar fossils, and formerly known as the Upper Bagshot Sands of the Hampshire basin. The section is continued without any break into the Lower Headon.

The next section described was that exposed in the cuttings for the new line from Brockenhurst to Christchurch, and here great changes in the relative thicknesses are seen, confirming the view that the Barton formation is the local deposit of a limited estuary. The *Chama*-bed remains 18 ft. thick, but the shell-bed thickens to 22 ft. and the drab clay with *Septaria* is only 10 ft. The underlying greenish compact clay looks like Lower Barton, but may belong to the Middle. The Upper Bartons are much weathered and unfossiliferous, but the *Paludina*-beds of the Lower Headon do not appear for 3900 yards east. Some of these, 2330 yards west of the Brockenhurst road, are violently contorted.

The Alum-Bay section was then compared with those previously given, and the authors also noticed the Bracklesham, Stubbington, and Hunting-Bridge sections to show the transitional character of the highest of the Bracklesham beds. The paper concluded with an analysis of the fauna, and carefully revised and tabulated lists.

Mr. Monckton, in his Appendix, stated that in the London basin the Barton beds are represented by the Upper Bagshot Sand, a mass of yellow or nearly white sand without clay-beds, though often loamy. Its greatest proved thickness is  $228\frac{1}{2}$  feet, and the base is marked by a very persistent bed of pebbles.

Its extent is considerably greater than is shown on the Geological Survey map.

Casts and impressions of shells are abundant in some places, but recognizable species have only been found at Tunnel Hill near North Camp Station, Aldershot. A large collection from this place has been made by Mr. Herries and by the author.

April 11.—W. T. Blanford, LL.D., F.R.S., President,  
in the Chair.

The following communications were read:—

1. "On the Lower Beds of the Upper Cretaceous Series in Lincolnshire and Yorkshire." By W. Hill, Esq., F.G.S.

The Red Chalk which forms the basement-bed of the Upper Cretaceous in Lincolnshire and Yorkshire is a continuation of the Hunstanton Limestone. Its thickness increases in South Lincolnshire to thin away again in the north of that county; but it again increases north of the Humber for a while. Near its most north-westerly exposure on the Yorkshire Wolds the red colour is lost; but *Inoceramus sulcatus* and *Belemnites minimus* are found in a dirty yellow-coloured material of trifling thickness. Eastwards it regains its red colour and thickness, so as to be upwards of 30 feet at Speeton, where also it is less calcareous. This section was described in detail, and the results compared with those of other writers. The author speculated upon the probable limits of the Upper Cretaceous sea at this period on evidence mainly based upon the amount of matter of inorganic origin. He noted that *Am. interruptus* has been found at Witheall, *Am. rostratus* at South Cave, and *Am. ? auritus* at Wharram Grange.

The base of the Chalk Marl through Lincolnshire continues to



be marked by a bed of compact limestone, which is the representative of the "sponge-bed" of Hunstanton. This can also be traced in Yorkshire as far as the north-western extremity of the Wolds. Above this a few feet of grey gritty chalk retain the character of the "*Inoceramus*-bed" throughout the area above mentioned. At the north-western extremity of the Wolds the main mass of the Chalk Marl has diminished in thickness, but more than recovers this at Speeton, where, according to the chemical and microscopical evidence, there is a complete passage from the "Gault" to the "Chalk Marl." The peculiar development of the latter at Speeton was very fully described. No bed such as the Cambridge Greensand or the Chloritic Marl can be taken as a line of separation.

Throughout Lincolnshire and Yorkshire certain courses of grey-coloured chalk are recognizable on the horizon of the Totternhoe Stone: these are known collectively as the "Grey bed." Much comminuted shell and numerous *Pectens* characterize this bed, which is faintly recognizable even at Speeton. The "Grey bed" determines the upper limit of the Chalk Marl. The equivalents of the Grey Chalk vary less in thickness throughout the area than those already described. Certain lithological characters, which first begin to manifest themselves in the marly beds just above the Totternhoe Stone in Norfolk, become greatly developed in South Lincolnshire, and throughout that county, as in Norfolk, the Grey Chalk is usually of a marly nature. In Lincolnshire there is much red coloration on this horizon. The occurrence of *Belemnitella plena* in Lincolnshire has been recognized. The band of bluish black clayey material in which it occurs at Barton continues throughout Yorkshire, but no Belemnite has yet been found. Allusion was made to the characteristic features towards the base of the Middle Chalk. Lists of fossils were given, and a new species of *Holaster* (*H. rotundus*) was described. Numerous chemical analyses and microscopic details of structure were also given.

2. "On the Cae-Gwyn Cave, North Wales." By Henry Hicks, M.D., F.R.S., F.G.S.; with an Appendix by C. E. De Rance, Esq., F.G.S.

The author gave an account of the exploration of the cavern during the latter part of 1885, and during 1886-7. He considered that the results obtained during that time proved conclusively that there was no foundation for the views of those who contended that the drift which covered over the entrance and extended into the cavern was *remanié*, but they proved that the deposits which lay over the bone-earth were *in situ*, and were identical with the normal glacial deposits of the area. These deposits had once extended continuously across the valley, and the cavern (400 feet above Ordnance Datum) had consequently been completely buried beneath them.

The cave must have been occupied by animals during the formation of the bone-earth, before any of the glacial deposits now found there had accumulated, and a thick floor of stalagmite had covered



this "earth" before the cavern had been subjected to water-action. This action had broken up the floor, and completely resorted the materials, and added sandy and gravelly material to the deposits; this sand and gravel had been examined by Prof. Boyd Dawkins, who found that it agreed in every particular with the glacial sand and gravel occurring in the valley a little way above. The large limestone blocks in the cavern had also been evidently disturbed by water-action; they were invariably found in the lowest deposits, and were covered over by laminated clay, sand, and gravels. The author considered it certain that the caverns had been completely filled with these materials, and in the case of the Cae-Gwyn Cave they appeared to have been conveyed mainly through the entrance recently discovered under the drift. The stratification at this entrance was so marked, and could be traced so continuously inwards over the bone-earth, that there could be no doubt that this was the main entrance. There was not the slightest evidence that any portion of the material had been conveyed in through a swallow-hole, and the conditions witnessed throughout were such as to preclude any such idea.

The author quoted a Report by Dr. Geikie, who considered that the wall of the cavern had given way, but before the deposition of the glacial deposits, which were subsequently laid down against the limestone bank so as to conceal this entrance to the cavern.

In conclusion, he referred to the presence of reindeer remains in these caves, in conjunction with those of the so-called older Pleistocene mammalia, proving that these had reached the area long before the period of submergence, and evidently at an early stage in the Glacial period. It was important to remember that reindeer remains had been found in the oldest river-gravels in which implements had been discovered. Man, as proved by the implements discovered, was also present at the same time with the reindeer, and it was therefore natural to suppose that he migrated into this area in company with that animal from some northern source, though this did not preclude the idea that he might also have reached this country from some eastern or southern source, perhaps even at an earlier period.

Mr. De Rance, in an Appendix, confirmed Dr. Hicks's observations as to the identity of the deposits outside the cavern with those in its interior, and noted the occurrence of limestone blocks in the lower deposits, not merely at the spot where the supposed broken wall was situated, but also throughout the whole tunnel. He stated that the sand bed forming the uppermost cave-deposit resembled the sand associated with gravels in a pit 400 yards east of the cave at a slightly higher level. The drift exposed in this gravel-pit he believed to be of the same age as that of the Mostyn and Bagillt pits to the north, which were undoubtedly overlain by Upper Boulder-clay. The westerly termination of the bone-earth outside the cave had not been determined, which he regretted; but traces of bone had been found at a point five feet from the overhanging ridge of the cave.

LVI. *Intelligence and Miscellaneous Articles.*

## ON THE THERMAL CONDUCTIVITY OF HARD AND SOFT STEEL.

BY F. KOHLRAUSCH.

WE know from Mousson\*, and more particularly from the thorough investigations of Barus†, that the electrical conductivity of steel depends on its hardness, and, as Barus found, so closely that the hardening of a soft steel bar can increase its conductivity two or three times.

If, now, the relation proved by Wiedemann and Franz‡ to exist for different metals, that a metal conducts heat and electricity almost equally well, holds also for the influence of mechanical preparation, or molecular aggregation, it is to be expected that the thermal conductivity of steel will be powerfully influenced by its degree of hardness.

The probability of an affirmative answer to this question follows from the fact that the statements respecting the conductivity in iron and steel lie much further apart than with other materials. Kirchhoff and Hansemann find in three kinds the numbers 0.096, 0.137, and 0.142 grm. cal./cm. sec. The authors do not discuss the possible causes of this great difference, and seem to assign it principally to the different proportions of carbon and silicon. An observation, however, from which follows that the magnetic coercive force of the worse conducting iron was the greater, points to an influence of the hardness on the conducting power§.

I used two cylindrical-turned and well polished bars of 1.2 cm. diameter, 30 cm. in length, and 270 grammes in mass||. They were cut from the same piece: one was heated and cooled slowly, while the other was glass-hardened.

Sensitive hands can at once tell by holding the cold bars, that the soft steel conducts better than the hard. A lecture-experiment on the rate of melting of wax or something similar is sufficient to show the difference¶.

In order to determine the numerical relations approximately, I made a few measurements by Despretz's method, which was also that adopted by Wiedemann and Franz. The end of the bar was heated by steam, and after the temperature was stationary the excess of temperature  $u$  over the surrounding air was measured in three equidistant sections\*\*.

\* Mousson, *Neue Denkschr. der Schweiz. Gesellschaft*. xiv. p. 1 (1855).

† Barus, *Wiedemann Ann.* vol. vii. p. 399 (1879).

‡ Wiedemann and Franz, *Pogg. Ann.* vol. lxxxix. p. 531 (1853).

§ Kirchhoff and Hansemann, *Wied. Ann.* vol. xiii. p. 417 (1881).

|| The soft bars weighed 271, and the hard one 268 grammes.

¶ The following method is perhaps the most convenient. The bottoms of the bars to be compared are placed in a freezing-mixture, of alcohol and snow for instance, and the height is observed up to which a deposit of moisture or of ice takes place.

\*\* As the conductivity of iron from 0° to 100° does not vary more than 2 per cent. according to Lorenz, it was needless for my purposes to take this into account.

For this purpose a thermoelement was used consisting of very thin German silver and iron wire soldered together; these wires loaded with a small weight were so laid above the horizontal bar that the junction was uppermost. At a distance of about 10 cm. from this place both wires were soldered to thin copper wires, which by means of a commutator were in connexion with a reflecting-galvanometer. When the thermoelement was in contact with the bars, the temperature of the junction was obviously proportional to that of the section in question.

Loss of heat by radiation was prevented by screens and coatings of wadding. The junction at the copper wires was sufficiently near to the temperature of the air for my purposes. As the element showed accidentally an electrical force nearly proportional to the difference of temperature, the deflection of the galvanometer was put for the difference of temperature.

In order to get an approximate statement as to the absolute conductivity, the loss of heat to the surroundings was measured by heating the entire bar and then observing its gradual decrease of temperature with the thermoelement suspended over it. These observations were made by Mr. Sheldon. According to this the temperature decreased in ten minutes in the ratio 1·67:1; the external conductivity for temperature in one second is therefore  $1·67/600 - 1 = 0·00086$ .

This number multiplied by the specific heat 0·117\*, and the density 7·9, gives the external thermal conductivity reduced to unit section = 0·00080, so that, denoting the internal conductivity by  $k$ , we have

$$\frac{d^2u}{dx^2} = \frac{0·00080}{k} u.$$

Considering now the temperatures  $u_1, u_2, u_3$  of three sections distant from each other by the length  $l$ , putting

$$\frac{u_1 + u_3}{2u_2} = n,$$

the conductivity  $k$  is known to be

$$k = 0·0008 \left[ \frac{l}{\log \text{nat} (n + \sqrt{n^2 - 1})} \right]^2.$$

We obtain:—

for $l$	=	4	5	8	8 cm.
$k$ hard	=	0·063	0·062	0·061	0·062
$k$ soft	=		0·106	0·118	0·111

Hence in the mean,

$$k \text{ hard} = 0·062 \text{ grm. cal./cm. sec.}$$

$$k \text{ soft} = 0·111 \text{ „ „ „}$$

*The conductivity of soft steel is thus almost 80 per cent. greater than*

\* According to Regnault, who puts the specific heat of hard steel at one per cent. higher, and the density by as much lower than with soft steel.—Pogg. *Ann.* vol. lxxii. p. 73 (1877).

that of hard. And as heating to temperatures like that of boiling water produces an appreciable annealing, the conducting power of the hard steel determined at a lower temperature would have been even less.

The electrical conductivity of the two bars was further determined by transmitting a constant current, a branch of which was sent by means of two knife-edges through a sensitive galvanometer in a current of 5000 to 10,000 ohms resistance. The factor of reduction to absolute measure was determined by means of a Clark's element. The conductivities referred to mercury are, according to a measurement of Mr. Sheldon,

$$\kappa \text{ hard} = 3.3, \quad \text{soft} = 5.5.$$

A heated and slowly cooled bar of wrought iron of the same dimensions was investigated. Its conducting power was about 40 per cent. higher than that of soft steel:

$$\kappa \text{ soft wrought iron} = 7.6.$$

That its conducting power was greater in a similar ratio is shown by an experiment in the freezing mixture (see p. 448, note). The height of deposit in hard steel amounted to 72 mm., and in soft steel to 92 mm., and in soft iron to 110 millim.

We have thus found for the ratio of the thermal conductivity  $k$  to the electrical conductivity  $\kappa$ :—

$$\frac{k}{\kappa} = \frac{\text{Hard Steel. } 0.062}{3.3} = 0.019; \quad \frac{\text{Soft Steel. } 0.111}{5.5} = 0.02.$$

The corresponding numbers with Kirchhoff and Hansemann are for 15°:—

$$\frac{k}{\kappa} = \frac{\text{Bar No. I. } 0.1418}{6.803} = 0.0208; \quad \frac{\text{No. II. } 0.0964}{4.006} = 0.0237; \quad \frac{\text{No. III. } 0.1375}{6.569} = 0.0209.$$

As my determinations of the thermal conductivity can only lay claim to an approximate measurement, the agreement of  $\frac{k}{\kappa}$  cannot be expected to be closer.

While thus the conductivity of different and differently heated iron and steel may be different, the ratio of the conductivity for heat and for electricity seems to remain about the same.—Wiedemann's *Annalen*, No 4, 1888.

#### MOUNTAIN FORMATION.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

In your issue of March (p. 210) Mr. T. Mellard Reade publishes a very suggestive paper on the "Geological Consequences of the Discovery of a Level-of-no-Strain in a Cooling Globe," in which he maintains the untenableness of the contractional theory of moun-



tain-formation. I have nothing to say on this thesis, but I wish to draw attention to what I conceive to be a mistake in his view of Appalachian structure. Great chains, as we all know, consist usually of a granite or metamorphic axis flanked on either side by tilted and often crumpled strata. Now Mr. Reade seems to think that the strata were pushed back and crumpled by the protruded axis. The Appalachian seems inconsistent with this view, but Mr. Reade thinks only seemingly so, because the crumpled strata on the *eastern flank* have been completely carried away by erosion.

Now American geologists believe they have good reason to think that the metamorphic region of the Appalachian does not belong to the Appalachian chain proper at all, but *existed as land long before the Appalachian was born*, viz. at the very beginning of the Palæozoic Era. The evidence of this is, that the very lowest Palæozoics lie everywhere unconformably on the eroded edges of the crumpled Archæan, and even the outlines of the old Cambrian shore-line can be traced. The Appalachian was not formed until the end of the Palæozoic.

Perhaps it may not be amiss to call attention also to the structure of the coast-range of California. It is strongly crumpled, with at least five anticlines and corresponding synclines\*, and yet in many places no sign of a granite or metamorphic axis.

Very truly,

JOSEPH LECONTE.

Berkeley, Cal.,  
March 29, 1888.

ON THE ACTION OF LIGHT ON THE ELECTRICAL CONDUCTIVITY  
OF THE HALOID SALTS OF SILVER. BY SVANTE ARRHENIUS.

Two parallel silver wires were coiled, at a distance of 2 millim., about a rectangular glass plate ( $4 \times 5$  centim.), then painted with an ammoniacal solution of chloride or bromide of silver, and slowly heated until the water and ammonia had evaporated, so as to form a thin skin of chloride or of bromide of silver. The plate was covered with a screen with a slit 1 centim. in breadth, and thereupon placed in various parts of a spectrum 7.2 centim. in length. The silver wires were introduced into the circuit of a battery of 40 Clark's elements, in which was a very sensitive reflecting-galvanometer (1 div.  $= 7 \times 10^{-11}$  amp.). Without illumination the galvanometer gave a constant deflection. With illumination this was greater, but returned to its former value on darkening. When the width of the slit which produced the spectrum was respectively 0.60, 0.473, and 0.24 centim. in breadth, the action of the light for each part of the spectrum was proportional to the intensity of the light. For the various colours, the action increased from the red to the line G, first of all slowly, then more rapidly, and sank then to the ultra-violet in the reverse direction. The action is thus not

\* Amer. Journ. Science, vol. ii. p. 297 (1876).

due to the heating, but corresponds to the strength of the photo-chemical action. According to the author, the velocity of the action of two bodies, whose specific electrical conductivities are  $\lambda$  and  $\lambda_1$ , is proportional to the product  $\lambda \cdot \lambda_1$ ; and thus in a mixture of gelatine and chloride of silver the action must be nearly proportional to the conductivity of chloride of silver, since  $\lambda$  must be regarded as tolerably constant for the former. If sensitisers are present,  $\lambda$ , the conductivity of the sensitiser, is variable, and a maximum is obtained for the colour which exerts an influence, and which corresponds to a sharp absorption-band, together with the other maximum without sensitiser.—*Wiener Berichte*, vol. xevi. p. 831 (1887); *Beiblätter der Physik*, No. 2 (1888).

---

#### GLACIAL MOTION.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

In your issue of February last (p. 156) Mr. Deeley brings forward what he thinks is a new theory of Glacial Motion; but, if I am not greatly mistaken, it does not differ essentially from that of James Thomson. In regard to this theory, in 1882 I wrote as follows\* :—  
 “Some time ago James Thomson brought forward a theory which deserves far more attention than it has yet received. Thomson shows that the fusing-point of ice is lowered by pressure, and therefore that ice at or near its freezing-point (as is the fact in glaciers) *is promptly melted by pressure*. Now it is obvious that in the differential motion of a glacier, whatever point, at any moment, receives the greatest stress of pressure must melt and give way, and the stress being relieved, must immediately again refreeze. Meanwhile, by change of relative position of parts, the stress is transferred to some other point which in its turn melts, gives way, and is refrozen; and again transfers its stress to still another point, and so on. If we compare this theory with Tyndall's, in both cases the ice gives way at the point of greatest stress: in the one case the stress is a stress of *tension*, in the other of *pressure*; in the one case the giving way is by *fracture*, in the other by *melting*. The differential motion in the one case is by *fracture*, change of position and regelation; in the other by *melting*, change of position and regelation.”

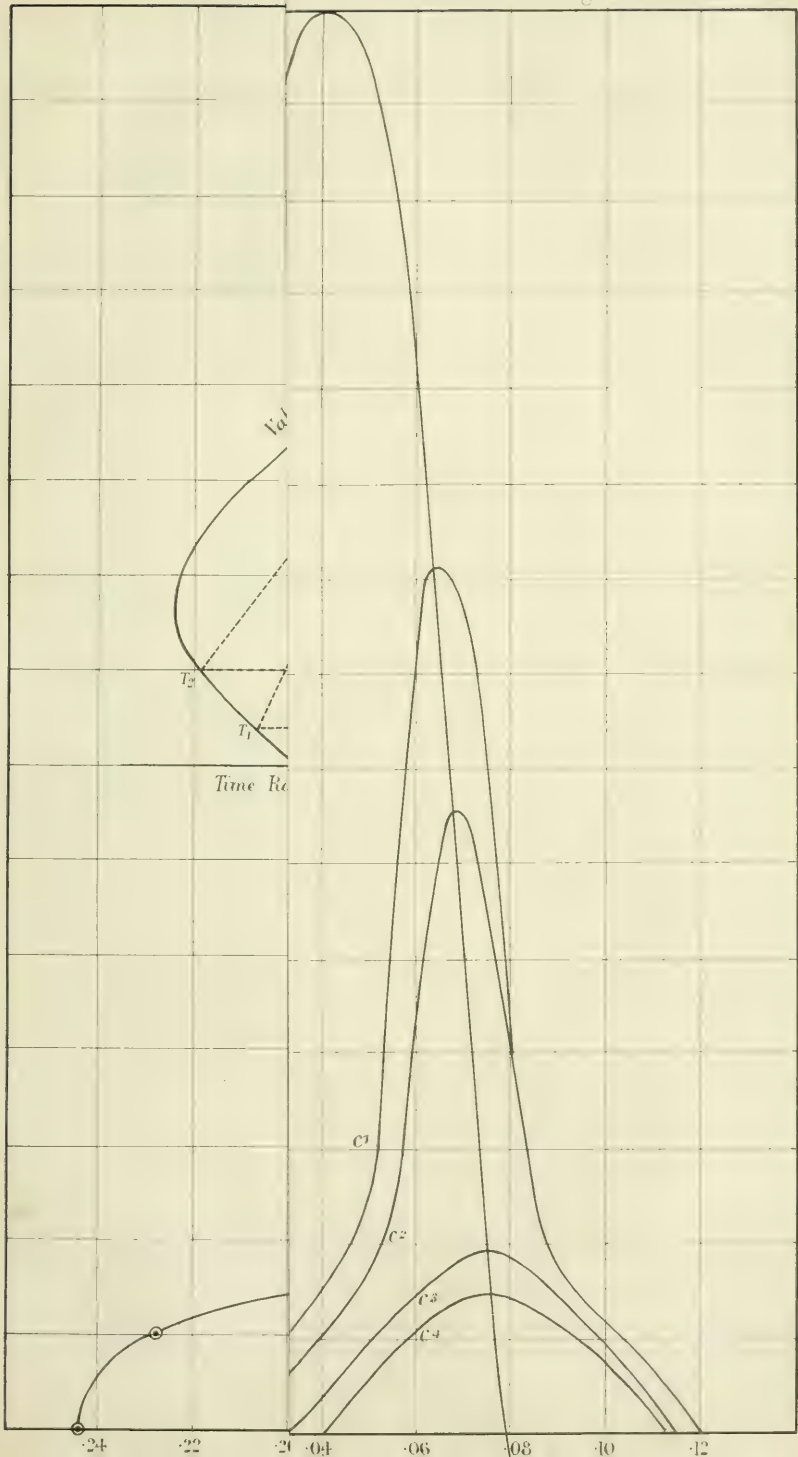
These of course are not Thomson's words, but only a condensed statement of my own of the view I got from studying Thomson.

Very respectfully,

JOSEPH LECONTE.

Berkeley, Cal.,  
 March 28, 1888.

\* ‘Elements of Geology,’ p. 603.







THE  
LONDON, EDINBURGH, AND DUBLIN  
PHILOSOPHICAL MAGAZINE  
AND  
JOURNAL OF SCIENCE.

[FIFTH SERIES.]

JUNE 1888.

LVII. *The Variation of the Coefficients of Induction.* By W. E. SUMPNER, B.Sc., Assistant in the Physical Department, Central Institute, South Kensington\*.

[Plate III.]

1. **T**HERE are three ways of defining the coefficient of self-induction, which lead to the same result if the magnetic permeability of the medium is a constant quantity, but which lead to three different results if it is a variable one, as in the case of iron.

The coefficient of self-induction  $L$  of a coil of wire through which a current  $C$  is passing may be defined as the ratio between the back electromotive force and the time-rate of change of the current  $C$  to which it is due; or it may be defined as the ratio between the flux of induction through the coil and the current producing it; or it may be defined with reference to the electrokinetic energy possessed by the current  $C$ .

The three definitions are expressed by the equations

$$(1) \quad e = L_1 \frac{dC}{dt},$$

$$(2) \quad N = I_2 C \quad \text{and} \quad e = \frac{d(L_2 C)}{dt},$$

$$(3) \quad T = \frac{1}{2} L_3 C^2.$$

In these equations  $e$  is the back electromotive force produced by varying the current  $C$ ,  $N$  is the number of lines of

\* Communicated by the Physical Society: read 14th April, 1888.

force passing through the coil,  $T$  is its kinetic energy, and  $L$  its coefficient of self-induction.

2. If the magnetic medium is air,  $L_1$ ,  $L_2$ , and  $L_3$  are identical. If the medium be wholly or partially composed of iron, this is no longer the case. The values of  $L$  differ from each other, and vary with  $C$ . The value of  $L_1$  can easily be found in terms of  $L_2$  from the equation

$$L_1 = L_2 + C \frac{dL_2}{dC},$$

which is an immediate consequence of the first two equations of definition.

We see that  $L_1 = L_2$  only when  $C=0$  or when  $\frac{dL_2}{dC}=0$ , and that  $L_1$  is greater than  $L_2$  for small currents where  $L_2$  is increasing with  $C$ .

Since  $\mathfrak{H}$ , the magnetizing force, is proportional to  $C$ ; and since  $N$  is a measure of  $\mathfrak{B}$ , the average value of the flux of induction per unit area, we see that  $L_2$  is directly proportional to  $\mu$ , the average value of the coefficient of magnetic permeability for the magnetizing force represented by the current  $C$ .

If, therefore, the medium be wholly of one kind,  $L_2$  will be a measure of the permeability of that medium. In any case, however, if we know the relation connecting  $\mathfrak{B}$  with  $\mathfrak{H}$ , we shall be able to determine the way in which the coefficients vary with the magnetizing force.

If  $OPK_1'$  (see fig. 1) be the curve connecting  $\mathfrak{B}$  with  $\mathfrak{H}$  (or  $N$  with  $C$ ), the value of  $L_2$  for any point  $P_1$  on it corresponding with the current  $OC_1$  and the flux of induction  $ON_1$  will be represented by the tangent of the angle which the line  $OP_1$  makes with the line  $OC_1$ .

Since

$$L_1 \frac{dC}{dt} = e = \frac{d(L_2 C)}{dt} = \frac{dN}{dt},$$

we have

$$L_1 = \frac{dN}{dC} \propto \frac{d\mathfrak{B}}{d\mathfrak{H}},$$

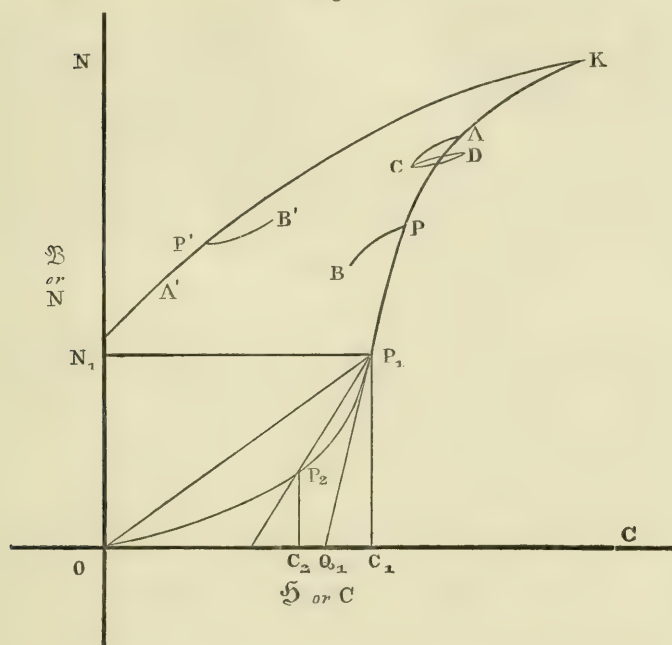
and  $L_1$  will consequently be represented by the tangent of the angle  $P_1Q_1C_1$  which the tangent to the curve at  $P_1$  makes with the line  $OC_1$ .

Moreover,

$$L_3 = \frac{2 \int C dN}{C^2} \propto \frac{\int \mathfrak{H} d\mathfrak{B}}{\mathfrak{H}^2},$$

and will be represented by the ratio of the area enclosed by the lines  $ON_1$ ,  $P_1N_1$ , and the curve  $OP_1$  to the square on the line  $OC_1$ .

Fig. 1.



3. Maxwell's method of determining the coefficient gives  $L_2$ , because the quantity of electricity discharged through the galvanometer is proportional to the number of lines of force inserted by the establishment of the current  $C$  (or to the number removed by stopping the current), while the steady deflection due to a small derangement is proportional to  $C$  directly. The method therefore gives the ratio of  $N/C$  or  $L_2$ . Professor Ayrton has generalized this method by altering the current from one value to another, instead of establishing it from zero to its full value. The quantity discharged through the galvanometer is proportional in this case to  $N_1 - N_2$ , where  $N_1$  is the number of lines of force corresponding with the first current  $C_1$ , and  $N_2$  that corresponding with the other current  $C_2$ . If the steady deflection caused by a derangement from balance be taken when a current  $C$  is flowing through the coil, Maxwell's formula will give the ratio of  $\frac{N_1 - N_2}{C}$ ;

and if we multiply this by  $\frac{C}{C_1 - C_2}$ , we shall obtain the value of  $\frac{N_1 - N_2}{C_1 - C_2}$ , or the tangent of the angle which the chord  $P_1P_2$  makes with the line  $OC$ .

By observing the ratio  $\frac{C_1 - C_2}{C}$ , in addition to the ordinary quantities measured in Maxwell's method, it is possible to determine in this way the rate of slope of the curve of magnetization. If  $(C_1 - C_2)$  is very small compared with  $\frac{1}{2}(C_1 + C_2)$ , the value obtained by Prof. Ayrton's method is  $L_1$ ; but the result is practically the same if the curve of magnetization does not bend very much between the values of  $OC$  corresponding with  $C_1$  and  $C_2$ .

#### *Coefficients for small Currents.*

4. It follows from the shape of the initial part of the curve of magnetization that the value of  $L_2$  must at first increase with the current. The following experiments show that this is the case. A bar of best Swedish iron, 14 in. long by  $\frac{1}{2}$  in. in diameter, was bent into the shape of a horseshoe. Each end was surrounded by a bobbin,  $2\frac{1}{2}$  in. in diameter and  $4\frac{1}{2}$  in. long, wound with 400 turns of wire. The self-induction of the two bobbins in series was obtained by Maxwell's method. The battery used was an ordinary Leclanché cell. The swings were in most cases taken on breaking the circuit, since, if the galvanometer-key be pressed just before the battery-key is released, the error caused by a slight want of balance in the bridge is reduced to a minimum. For a similar reason, the deflection corresponding with a known derangement was always estimated from the difference between two deflections. The current flowing through the coil was calculated from the electromotive force of the battery and the resistances in the network. The following numbers represent the mean of several concordant values:—

A.	$L_2$ .	$L_2 - 2A$ .
·047	·0514	·0420
·056	·0539	·0428
·060	·0549	·0429
·065	·0554	·0424
·068	·0564	·0428
·079	·0577	·0423
·091	·0609	·0427
·107	·0634	·0420

In this table  $L_2$  is the coefficient of self-induction of the



coil in secohms, and  $A$  is the current in amperes flowing through it. The experiments are in fair accordance with the relation

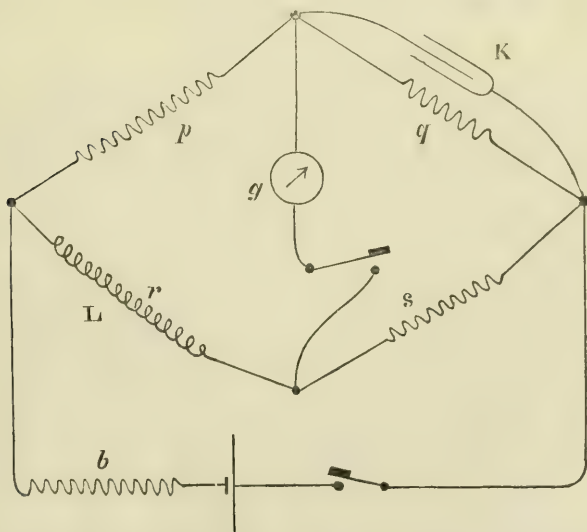
$$L_2 = .2A + .0425.$$

Since  $A$  is proportional to  $\mathfrak{H}$ , and  $L_2$  is proportional to  $\mathfrak{B}/\mathfrak{H}$ , the above experiments tend to show that the part of the curve of magnetization between the points corresponding with the currents .047 and .107 ampere is a parabola of the form

$$\mathfrak{B} = a\mathfrak{H}^2 + b\mathfrak{H}.$$

5. It was, however, desirable to see whether this relation would still hold good for smaller magnetizing forces. With this view the experiments were repeated some months afterwards in a somewhat different way. The coefficient was determined by comparing it with the capacity of a standard condenser. Two arms,  $p$ ,  $q$ , of a Wheatstone bridge (see fig. 2) consisted of doubly wound resistance-coils of 10,000 ohms each. The resistance  $g$  of the galvanometer used was also about

Fig. 2.



10,000 ohms. To the coil  $q$  was shunted a condenser of  $\frac{1}{3}$  microfarad capacity. The opposite branch  $r$  of the bridge contained the electromagnet whose self-induction was required. The resistance of the arm  $s$  was 5 ohms, and that of  $r$  was adjusted till there was balance. The battery used was

an accumulator. The currents were changed by inserting more or less resistance into the battery-circuit. If the bridge is balanced for steady currents, and the battery-circuit be opened, the swing  $\theta_1$  produced will be proportional to  $L_2 - Kps$ , where  $L_2$  is the coefficient required and  $K$  is the known capacity. If one of the terminals of the condenser be now disconnected from the bridge, and a second swing  $\theta_2$  be taken on breaking circuit, we have

$$\frac{\theta_2}{\theta_1} = \frac{L_2}{L_2 - Kps},$$

or

$$L_2 = \frac{\theta_2}{\theta_2 - \theta_1} Kps.$$

This method, although only comparative, has several advantages over Maxwell's absolute method. It is quicker and simpler; there are only two quantities to observe, and the readings may be taken immediately after each other. It is, moreover, not so necessary to have a good ballistic galvanometer. It will generally be best to work with the relations

$$p=q, \quad p+r=2g, \quad Kps=2L.$$

The best ratio,  $r/p$ , will be determined by the resistances and batteries available, and by the currents it is desirable to use. In the following experiments these relations were not adhered to, because the galvanometer was sufficiently sensitive, and because it was desirable to alter the currents flowing without rendering it necessary to readjust the bridge.

In all swing methods it is necessary to have the bridge well balanced for steady currents. A fine adjustment can be conveniently obtained by sliding a bare wire of suitable thickness round a terminal, since it is not generally necessary to know the resistance of the arm of which it is a part, and it can of course be placed in the arm of unknown resistance. I believe the device used in the Cavendish laboratory is to shunt one resistance-box by another. The balance of the bridge has to be so good that the heating caused by the momentary passage of the current is often sufficient to destroy it.

The annexed table indicates the results obtained with the electromagnet with the iron core. The swings were obtained by breaking the battery-circuit.

$b.$	$\theta_2.$	$\theta_1.$	$L_2.$	$A.$
0	688	508	·0637	·220
1	627	462	·0634	·200
5	444	321	·0606	·147
10	367	261	·0577	·110
20	230	160	·0547	·073
30	164	111	·0517	·055
100	49·5	29·75	·0419	·020
130	38·0	22·7	·0414	·016
200	24	13·5	·0381	·0105
230	20·5	11·0	·0360	·0092

The values obtained for  $L_2$  are slightly lower than the corresponding values obtained by Maxwell's method three months previously. This is probably due to the fact that the electromotive forces of the cells used were only approximately determined. The differences, however, are small, and may have been due to alterations in the state of the iron. The values for  $L_2$  obtained for small currents are seen to be much lower than those given by the equation

$$L_2 = \cdot 2A + \cdot 0425.$$

They are, however, very much higher than that of the coil with the iron core removed. The coefficient in this case was ·0028 secohms.

6. The coefficient was very much increased by keeping a piece of soft iron pressed against the poles of the electromagnet. As, however, under these circumstances, a large fraction of the magnetism remained on removing the magnetizing current, the values obtained depended upon the previous history. Thus the value of  $L_2$  obtained for a magnetizing current of about ·04 ampere varied between ·337 secohm and ·130 secohm, according as the flings were obtained on reversing the current, on breaking the battery-circuit, or on making it so as to include or exclude the permanent magnetism. The numbers obtained showed, moreover, that the curve of magnetization was not quite symmetrical with respect to positive and negative currents.

The following table gives the values of  $L_2$  obtained for different currents by the method of reversals. The resistances and electromotive forces used to obtain the last five observations were quite different from those used to obtain the first six.

A.	$L_2$ .	$\mathfrak{B}$ .	$\mu$ .	$\mathfrak{H}$ .
·0366	·184	665	780	0·852
·0275	·156	424	661	·640
·0220	·141	306	597	·512
·0147	·112	163	474	·342
·0110	·091	99	385	·256
·0055	·071	38·6	301	·128
·0029	·062	17·8	263	·068
·0044	·066	28·7	279	·102
·0100	·091	89·8	385	·233
·0200	·128	253	542	·465
·0400	·189	746	801	·931

7. As the magnetic circuit in this case was entirely composed of iron, it was easy to reduce the observations to absolute measure. The values of the induction  $\mathfrak{B}$ , the magnetic force  $\mathfrak{H}$ , and the magnetic permeability  $\mu$  were calculated in C.G.S. units from the formulæ

$$\mathfrak{B} = \frac{10^8}{nS} L_2 A, \quad \mathfrak{H} = \frac{4\pi n}{10l} A, \quad \mu = \frac{10^9 l}{4\pi n^2 S} L_2,$$

where  $A$  was the current in amperes,  $L_2$  the coefficient of self-induction in secohms,  $S$  and  $l$  the mean values of the sectional area and length of the magnetic circuit in centimetres, and  $n$  the number of turns.

The iron core was half an inch in diameter and 14 inches long, and the distance between the poles was 3 inches. The number of turns was 800. Whence

$$l = 17 \times 2\cdot54 = 43\cdot2, \quad S = \frac{\pi}{4} \left( \frac{2\cdot54}{2} \right)^2 = 1\cdot27, \quad n = 800.$$

If these values are substituted, we obtain

$$\mathfrak{B} = 98,700 L_2 A, \quad \mathfrak{H} = 23\cdot3 A, \quad \mu = 4,240 L_2.$$

The numbers obtained very approximately fulfil the following relations:—

$$L_2 = 0\cdot05 + 3\cdot9 A,$$

$$\mu = 210 + 720 \mathfrak{H},$$

$$\mathfrak{B} = 210 \mathfrak{H} + 720 \mathfrak{H}^2.$$

The values were obtained for reversals of magnetizing force whose semiamplitude  $\mathfrak{H}$  was greater than 0·06 and less than 0·9 C.G.S. units. Beyond these limits the value of  $\mathfrak{B}$  probably differs from that given by the above relation. By employing the secohmmeter of Professors Ayrton and Perry, it would have been possible to obtain values of  $L_2$  for much



smaller magnetizing forces than those used above, and therefore for fields very much weaker than that due to the earth's magnetism.

*Coefficients for large Currents.*

8. The coefficient of self-induction of a coil with an iron magnetic circuit can far more easily be obtained for strong magnetic forces when the magnetism is maintained by an independent magnetic field than when the magnetism of the iron has to be excited by the current through the coil itself. In a transformer, for instance, where there are two coils identically situated with respect to the iron circuit, the coefficients of self and mutual induction are in the ratio

$$n_1^2 : n_2^2 : n_1 n_2,$$

where the ratio of the turns on the first coil to that on the second is  $n_1 : n_2$ . This will be true whatever the state of magnetization. If, therefore, one coil is used to excite the magnetism, and the other to test it, the values of the coefficients can be obtained without the least difficulty. Again, it is easy to obtain by Maxwell's method the value of  $L$  for the armature of a Series dynamo when the field-magnet coils are used independently to excite magnetism in the armature. Experiments were made in February 1887, at Prof. Ayrton's suggestion, to determine the coefficient of self-induction of the armature of a Gramme dynamo of the A pattern. The numbers obtained by Mr. S. Watney and the writer were as follow :—

Amperes round field-magnets.	Self-induction, in secohms.
0.0	·0218
6.1	·0179
15.1	·0135
24	·0122
29	·0117

9. When, however, it is desired to determine the self-induction of the field-magnet coils of a dynamo when a strong current is flowing through them, several difficulties present themselves. If strong currents are to be maintained the resistances must be small, unless a large amount of power is to be wasted and unless very high electromotive forces are procurable. On the other hand, the self-induction to be measured is very large. The time-constant, which determines the rate at which the magnetism will change, will therefore be large; and unless a

sufficiently ballistic galvanometer is to be had, it is not easy to obtain  $L$  in absolute measure.

At the suggestion of Prof. Ayrton some experiments were made at the commencement of 1887 to determine the variation of the self-induction of the field-magnet coils of a Ferranti dynamo. The method\* used was the modification of Maxwell's method already alluded to. (The coefficient has been since compared with the capacity of a standard condenser by the swing method; the value obtained for a current of 0.01 ampere was 0.61 secohm, and for larger currents larger values were obtained.) When a strong current was flowing through the coils the resistance of the circuit was only a few ohms, and the coefficient was probably larger than 0.6. The time-constant was therefore generally larger than one tenth of a second, and the discharge could never be considered completed in less than a second. Probably no galvanometer would have been sufficiently ballistic under these circumstances to give good results in absolute measure. As, however, the only galvanometer conveniently situated with reference to the dynamos was one of the D'Arsonval type, with a short period and large logarithmic decrement even when on open circuit, the hope of any but comparative measurements was abandoned.

10. It is possible to calibrate any galvanometer for ballistic purposes by charging a condenser to a standard potential and discharging it through a resistance in series with the galvanometer to be calibrated. By suitably altering the resistance or the capacity the time-constant of discharge may be made to have any value. This was done with the D'Arsonval galvanometer, in order to interpret the results obtained. A condenser of 18.6 microfarads capacity was charged by a Latimer-Clark's cell, and then discharged through the galvanometer (whose resistance was 700 ohms) in series with a variable resistance,  $R$  ohms. The time-constant of discharge was therefore

$$18.6(R + 700)10^{-6} \text{ seconds,}$$

and the ratio of this to 1.66 was the ratio of the time-constant to the period of the galvanometer, and is denoted in the following table by the letter  $\rho$ . Observations were made of the value of the first swing of the galvanometer-needle for many different values of  $R$ . A few of these are given below.

\* This method, due to Prof. Ayrton, will be found more fully described, together with several other methods, in a paper by the present writer on "The Measurement of Self-Induction, Mutual Induction, and Capacity," Journ. Soc. Tel. Engineers, May 1887.

R.	$\rho$ .	First Swing (mean).
0	·008	293·5
1000	·019	291·2
5000	·064	269·0
10000	·120	234·0
50000	·568	105·5
100000	1·128	61·5
200000	2·248	36·0
300000	3·368	25·0

Thus, for the particular galvanometer tested, the throw produced by a given discharge when the time-constant is one tenth of the period is only about 80 per cent. of that produced by the instantaneous discharge of the same quantity; and when the time-constant is equal to the period, the throw is only about 20 per cent. of the corresponding throw for instantaneous discharge. Experiments since made with a high-resistance Thomson astatic galvanometer, with a period of 10 seconds, have yielded practically the same results. In the actual experiments on the Ferranti field-magnet coils, it would have been possible to interpret the meaning of the galvanometer-swings if the time-constant of discharge had had any fixed value, or if this value had been known; but the resistances and self-induction were continually varying, and the self-induction was always unknown. It would probably have been impossible even to compare the results with each other had it not been for the fact that, as the currents increased, the resistance and self-induction of the circuit diminished simultaneously, so that the time-constant of discharge tended to remain fixed in value.

11. The experiments on the dynamo coils were carried out by Messrs. Rossiter and Watney together with the writer.

The electromotive force used was obtained from accumulators, and amounted to about 100 volts. The bridge was kept balanced for steady currents. The only resistance altered was that in the battery-circuit. The currents were changed by switching resistances into or out of the battery-circuit. If  $Q$  is the quantity of electricity discharged through the galvanometer when the current in the coil changes from  $C_1$  to  $C_2$ ,

$$L_1 = K \frac{Q}{C_1 - C_2},$$

where  $K$  is a function of resistances only, and is independent of the resistance of the battery branch when the bridge is balanced

for steady currents. If the discharge  $Q$  produces a throw  $\theta$ ,

$$Q = k\theta,$$

where  $k$  depends, among other things, on the time-constant of discharge, and therefore varies with the self-induction and with the resistance in the battery branch. We may, however, regard  $k$  as being approximately constant, and we may

accordingly take the values of  $\frac{\theta}{C_1 - C_2}$  to represent the values of the coefficient for the current  $\frac{1}{2}(C_1 + C_2)$ . In a series of experiments the currents were changed by successive steps from a small value to about 13 amperes, and decreased through the same stages in inverse order. The cycle was repeated many times, both for positive and for negative currents; and the numbers given in the following table are a fair sample of the results obtained.  $L_i$  and  $L_d$  are the values of  $\theta/(C_1 - C_2)$  for increasing and decreasing currents respectively.  $A$  is the value in amperes of  $\frac{1}{2}(C_1 + C_2)$ .

A.	$L_i$ .	$L_d$ .
0.21	98	137
0.69	115	143
1.13	118	133
1.65	126	131
2.27	126	128
2.75	131	132
3.04	132	137
3.39	113	111
3.91	113	111
4.60	104	100
5.60	106	101
7.15	78	72
9.70	61	51.5

The numbers obtained, although not altogether satisfactory, leave no doubt about the way in which the coefficients change. For increasing currents  $L_i$  at first increases and then diminishes, while for decreasing currents  $L_d$  begins at a lower value than the corresponding value of  $L_i$  and continually increases as the current is decreased. This is exactly what is to be expected when it is remembered that the value of  $L$  is represented by the slope of the curve of magnetization. If the curve connecting  $\mathcal{B}$  with  $\mathcal{H}$  be like that indicated in fig. 1, the curve connecting  $d\mathcal{B}/d\mathcal{H}$  or  $L$  with  $\mathcal{H}$  will be such that for increasing values of  $\mathcal{H}$  the ordinates  $L$  will at first increase and then diminish, while for decreasing values of  $\mathcal{H}$  the ordinates will continually increase. It is noteworthy that a discontinuity will occur at the cusp  $K$ , and the value of  $L$



will suddenly diminish as the change of current alters from an increase to a decrease, or *vice versâ*.

The general shape of the curve connecting  $L$  with  $\mathfrak{H}$  will be the same whether the value of  $\mathfrak{H}$  has been diminished to zero from positive or from negative values before the experiments are made. This follows from the fact that the slope at a point on a Ewing's cycle increases from each cusp to the point of inflexion. The coefficient  $L$  will be a maximum for those values of the magnetizing current  $C$  at which the points of inflexion occur. As the cycle only has one point of inflexion on each branch, the two parts of the curve connecting  $L$  with  $C$  will each have one maximum point, and one only. The two maximum values of  $L$  will not, however, occur at the same value of the current. They will occur at a small positive value of  $C$  for currents changing in the positive direction, and at a small negative value of  $C$  for currents changing in the negative direction. From this it follows that for increasing currents,  $L$  will at first increase and then diminish; while for currents decreasing to zero,  $L$  will continually increase. This statement must be modified for small cycles, which do not generally have points of inflexion. Here the coefficient will increase continually from one cusp to the other, and decrease discontinuously on rounding each cusp.

Experiments were also made on the field-magnets of a Gramme dynamo, the currents used varying in this case up to 30 amperes. The same kind of results were obtained.

12. Several remarkable effects were obtained, due to previous history of magnetization, which are easily explicable on reference to the researches of Professor Ewing\*.

Suppose the current has been altered until a point  $P$  (see fig. 1) on the curve of magnetization is reached. The effect of a small change of current will now depend upon whether the current be increased or diminished. If the current be changed in the same way as it was altered last, a point  $A$  on the curve of magnetization will be reached; while if it be changed in the opposite direction, a point  $B$  will be reached. The slope of the line  $PA$  will be quite different from that of  $PB$ , and as these slopes represent the values of the coefficient of self-induction at the point  $P$ , it follows that this coefficient has always two distinct values whatever the state of magnetization and however that state has been attained. The curves obtained by Professor Ewing imply that the coefficient is greater for a change of current in the same direction as the last than for one in the opposite direction; for if  $P$  be any

\* "Experimental Researches in Magnetism," Phil. Trans. part ii. 1885.

point on the ascending curve of magnetization, and  $P'$  any point on the descending curve, the slopes  $PA$  and  $P'A'$  correspond with changes of current in the same direction as those immediately preceding, and these slopes are steeper than those of  $PB$  and  $P'B'$ , which correspond with reverse changes of the magnetizing force.

If the current be changed between the two values corresponding with the points  $P$  and  $A$ , and the swings of the ballistic galvanometer observed for each alternation, the first fling will correspond with the slope  $PA$ , if the magnetization has been increased up to the point  $P$ . All the succeeding swings will, however, correspond with more gradual slopes,  $AC$ , because the magnetization owing to hysteresis will not return to the state  $P$ , but will proceed around a Ewing's cycle between the two points  $A$  and  $C$ , if the current be alternated between the two values considered. In the case of a transformer, two or three successively diminishing swings were often observed before the changes became cyclic. Whether on the ascending or descending portions of the magnetization-curve, the first fling should be greater than the succeeding ones if it corresponds with a change of current of the same kind as that immediately preceding.

The following experiments on the Ferranti field-magnets illustrate these remarks:—

Previous history.	Current Change.	Successive Swings.
C increased to +12 amperes, and diminished to 5·86.	5·86 to 3·56	−232 −205
	3·56 to 5·86	+193 +192
C diminished to 0, and raised to 3·56.	3·56 to 5·86	+230 +197 +195
	5·86 to 3·56	−209 −205
C increased to 4·86, and diminished to 3·13.	3·13 to 4·86	+149 +149
	4·86 to 3·13	−151 −152
C increased to +13, and diminished to 4·86.	4·86 to 3·13	−180 −157 −157
	3·13 to 4·86	+153 +152
C diminished to 0, and increased to 3·13.	3·13 to 4·86	+189 +153 +153
	4·86 to 3·13	−162 −159

The swings succeeding the first are seen to be all very nearly equal. The negative swings are slightly greater than the positive ones, because the current flowing is smaller, the resistances in circuit larger, and the time-constant of discharge smaller. Although the amount discharged is the same for

the two cases, the swing which measures it will be greater in that case for which the time-constant is the smaller.

13. Evidence of hysteresis was obtained in a very marked way from a 2 H.P. transformer lent to Professor Ayrton by Mr. Kapp. The coefficient of self-induction of the primary coil was measured when different currents were traversing the secondary. The magnetizing current (denoted by  $A$ ) was obtained from accumulators, and varied up to 10 amperes. It was sent in both directions, and its value was read by an Ayrton and Perry ammeter. The coefficient was obtained either by comparison with a standard condenser by the swing-method already described or by the secohmmeter. The test-current used to obtain the coefficient was  $\cdot 037$  ampere for the swing method and  $\cdot 01$  ampere for the secohmmeter method. In the former case means were provided for sending the test-current round the primary in either direction at will, so that the swings obtained corresponded either with the slope PA (see fig. 1) or with the slope PB. If the total current through the transformer-coils were alternated between two values corresponding with the points A and C, the slope AC was found not only to be less than the preceding slope PA but greater than the succeeding slopes CD, and in some cases several successively diminishing values were obtained. The coefficients corresponding with the slopes PA, AC, and CD will be respectively denoted by the letters  $L_p$ ,  $L_r$ , and  $L_c$ . The values obtained could only be reproduced when the magnetic history of the iron was exactly repeated; and whatever the process of magnetization was, the numbers obtained at corresponding parts of successive cycles gradually diminished until the true cyclic values were obtained.

Thus the values obtained by the swing-method for the progressive coefficient  $L_p$  were  $\cdot 087$  secohm or  $\cdot 192$  secohm, according as the value of  $A$  had been diminished to zero from 1 ampere or from 6 amperes. When  $A$  was 1 ampere, values of  $L_p$  could be obtained varying between  $\cdot 074$  and  $\cdot 130$  secohm, according to the previous history, and simultaneously the values of the return coefficient  $L_r$  could be varied between  $\cdot 032$  and  $\cdot 053$  secohm. The values of the cyclic coefficient  $L_c$  (which were obtained by reversing the test-current several times before taking the swing) were, however, fairly constant, and were  $\cdot 029$  secohm when  $A$  was zero and  $\cdot 016$  secohm when  $A$  was 6 amperes.

In order to obtain a complete set of values for the coefficients  $L_p$  and  $L_c$ , it was necessary that the iron should go through the same magnetizing processes before any reading was taken. The magnetizing current was therefore diminished to  $-10$

amperes before each observation, and then increased to the particular value of  $A$  at which the value of  $L$  was observed.

The annexed table indicates the results obtained by the swing method :—

$A$ .	$L_p$ .	$L_c$ .
0	·243	·0290
2	·230	·0261
4	·083	·0193
5	.....	·0177
6	·016	·0155
8	·013	·0120
10	·012	·0097

The values obtained with diminishing currents (*i. e.* for the curve  $P'A'$ ) were slightly lower. They were arrived at by increasing the current to +10 amperes and then diminishing it to the value of  $A$ , at which  $L_p$  was required.

The values of  $L_c$  were much easier to obtain than those of  $L_p$ , for the slightest variation in the main magnetizing current  $A$  was sufficient to render it impossible to obtain  $L_p$  until the previous magnetizing processes were gone through again. Accumulators were used to provide a constant E.M.F. ; but it was nevertheless found difficult to keep  $A$  sufficiently constant, so that the test-swings for  $L_p$  were taken as quickly as possible to prevent values intermediate between  $L_p$  and  $L_c$  being obtained. These considerations partially account for the unsatisfactory nature of the results obtained with the Ferranti field-magnets ; for, at the time those experiments were made, the necessity of keeping the current quite steady until the swing measuring  $L$  was observed was not realized. The numbers given do not show the slight initial increase in the value of  $L_p$  for small increasing currents, but more recent experiments have done so.

The values of  $L_c$  were also determined by means of the secohmmeter of Professors Ayrton and Perry. This instrument necessarily only measures coefficients for cycles of magnetism. The value of  $A$  was varied from zero to 10 amperes positive, thence to 10 amperes negative, and thence to zero again. The value of each coefficient was determined for three different speeds, and the mean taken. The value of the coefficient appeared to diminish slightly as the speed increased. (This effect has been observed in several cases when the magnetic circuit is wholly or partially composed of iron, and is probably due to magnetic lag.) The coefficients  $L_c$  obtained for a complete cycle of current values are given in the four



columns of the annexed table. The first two columns give the values obtained respectively for increasing and decreasing positive currents; the last two columns indicate those obtained for increasing and decreasing negative currents.

A.	Values of $L_c$ .			
0	·0231	·0231		·0231
2	·0221	·0193	.....	.....
4	·0167	·0134	·0194	·0147
6	·0150	.....	·0141	·0119
8	.....	·0105	.....	.....
10		·0090	·0099	

The values obtained are slightly lower than those obtained by the swing-method. This is accounted for by the fact that the amplitude of the cycle is less. With the secohmmeter the test-current was only ·01 ampere, while it was ·037 ampere with the former method. The value of  $L_c$  will be smaller, the smaller the amplitude of the cycle. The secohmmeter is so sensitive that it would have been easy by means of it to measure  $L_c$  for cycles of very small amplitude.

### Shape of Current Waves.

14. It is a matter of both theoretical and practical interest to determine the way in which currents change when the impressed electromotive forces are given at every instant of time and when the coefficients of self-induction are given in terms of the currents which are flowing. In calculations concerned with alternating-current problems it is usual to assume that the impressed electromotive forces are pure sine functions of the time, and that the coefficients of induction are constant quantities. These assumptions are more convenient than true. The coefficient of self-induction can be at once deduced from the curve of magnetization, and therefore can no more be expressed as a mathematical function of the current than electromagnetism itself. It therefore appears as if, in the treatment of such problems, graphical methods are to be preferred to analytical ones. If, in a simple circuit, the curve connecting the impressed electromotive force with time be given, together with the curve connecting magnetization with current, it is perfectly easy by purely graphical processes to obtain the curve connecting current with time. For we have

$$RC + \frac{dN}{dt} = E,$$

where  $E$  is the impressed electromotive force,  $N$  is the number of lines of force enclosed by the circuit,  $R$  is the resistance of the circuit,  $C$  the current flowing, and  $t$  the time at which the different quantities are evaluated.

Now, if we can neglect Foucault currents, or possible magnetic lag, or anything analogous which would make  $N$  directly dependent on time, we may put

$$\frac{dN}{dt} = \frac{dN}{dC} \frac{dC}{dt},$$

where  $\frac{dN}{dC}$  is the coefficient of self-induction,  $L$ . If we put

$$C_0 = \frac{E}{R}, \quad T = \frac{L}{R},$$

we obtain

$$\frac{dC}{dt} = \frac{C_0 - C}{T}.$$

Now  $C_0$  is the value of the current which would be flowing if there were no self-induction; and since  $E$  and  $R$  are given, it is possible to plot a curve having  $C_0$  for ordinates and time for abscissæ. Moreover, since the curve connecting  $N$  with  $C$  is given, it is possible by graphical processes to find another connecting  $\frac{1}{R} \frac{dN}{dC}$  or  $T$  with  $C$ . This curve (see Plate III. fig. 3) should be plotted, with the values of  $C$  for ordinates and the values of  $T$  as abscissæ. It will be found convenient to plot  $T$  in the negative direction. The time-ratio  $T$  will be in seconds if  $L$  is in secohms and  $R$  in ohms. The two curves should of course be plotted to the same scale for current and time.

The construction follows immediately from the equation

$$\frac{dC}{dt} = \frac{C_0 - C}{T},$$

and is as follows:—

Suppose  $P_1$  (see Pl. III. fig. 3.) be the given initial point on the curve connecting  $C$  with time. Project it parallel to the axes to  $T_1$  on the curve  $T$  and to  $Q_1$  on the curve  $C_0$ . Project  $Q_1$  parallel to the axis of abscissæ to  $R_1$  on the current axis. Draw from  $P_1$  a line parallel to  $R_1T_1$ , and choose a point  $P_2$  on it not far from  $P_1$ .  $P_2$  may be regarded as the next point on the current curve, and the process (which is indicated in Plate III.) may be repeated to obtain a third point  $P_3$ , and so on, until the whole curve connecting current with time is obtained.

15. The curves are very readily drawn on squared paper, and yield some remarkable results. If the impressed electromotive force  $E$  and the resistance  $R$  are constant quantities, the curve  $C_0$  will be a straight line parallel to the axis of time cutting the current axis in a point  $R$ . The inclination of the line joining  $R$  to any point  $T$  on the curve  $T$  will represent the rate at which the current is increasing when it has the value corresponding with the point  $T$ . Now, if the self-induction is constant or nearly constant, this rate of increase will be great at first but will continually diminish as time goes on. If, however, the self-induction be very variable, the result may be quite different and dependent to a great extent on the value of  $C_0$ . Suppose the self-induction to increase at first, and then to diminish. Unless  $C_0$  be very small, there will now be points of inflexion on the current curve. The current will increase very rapidly at first, slower afterwards, then more rapidly, and will finally attain its maximum slowly. If  $C_0$  be such as to magnetize the iron far beyond saturation, this effect may be very marked, and the time taken for the current to rise to a small fraction of its final amount may exceed that taken to rise through the remainder. The writer is indebted to Prof. Silvanus Thompson for the information that this fact has actually been observed when accumulators have been connected in series with the field-magnets of a dynamo and with an Ayrton and Perry dead-beat ammeter. The needle has been noticed to move very slowly at first, and then with great rapidity through the larger portion of the ultimate deflection. The greater the value of  $C_0$ , the quicker will the current attain a given fraction of its final value. This may be roughly accounted for by the fact that the mean value of the coefficient is less the greater the value of the final current.

16. When the electromotive force  $E$  is alternating, the coefficient  $L$  will not only be variable but two-valued. It will depend not only on the value of the current, but also on whether the current is increasing or diminishing. The curve  $T$  will therefore consist of two parts—one for increasing current, and the other for decreasing current. Although  $E$  and  $C_0$  may be pure sine functions,  $C$  will not be a simple sine function if the coefficient  $L$  varies. Suppose a current curve  $C'$ , drawn on the assumption that there is a constant coefficient of self-induction equal to the mean of the different values. The true curve  $C$  will, roughly speaking, differ from the curve  $C'$  in rising more rapidly when  $L$  is less than the average, and in rising less rapidly when  $L$  is more than the average. The general effect of the variation of the coefficient will therefore be to introduce ripples into the current curve.

It is noteworthy that these ripples are dependent not so much on the speed as on the value of the current.

17. Plate III. fig. 4 shows an example worked out on the basis of experiments actually made on the Kapp and Snell transformer already referred to. The resistance of the circuit is for simplicity assumed to be 1 ohm, so that the number which represents the coefficient of self-induction  $L_p$  in secohms also represents the time-ratio in seconds. The values of  $L_p$  given above were obtained for the primary coil when different currents  $A$  were traversing the secondary, and should be plotted, not with the values of  $A$ , but with the values of  $n_2 A / n_1$ , where  $n_1 / n_2$  is the ratio of the turns on the primary to those on the secondary. This has not been done, as it merely alters the scale of current. The values of  $L_p$  differ slightly for increasing and decreasing currents: this has been disregarded, as in the particular case taken the results would have been but very little altered. The electromotive force acting in the primary circuit (the secondary being open) is supposed to be a simple sine wave having a period of 0.16 second, and is represented by the curve  $C_0$ . The current curve obtained will depend for the first few alternations on the initial circumstances. The curves  $C_1$ ,  $C_2$ ,  $C_3$ , and  $C_4$  represent the first half-wave for different initial values of the current. Of these,  $C_3$  may be taken as the curve which periodically repeats itself and to which all the others will eventually come. It is not in appearance so markedly different from a sine curve as  $C_1$  and  $C_2$ . This is because the impressed electromotive force is not sufficient to produce a current capable of magnetizing the iron beyond the saturation-point, at which the value of the coefficient of self-induction begins to diminish. Otherwise the current curve would have sharp peaks in it, as indicated in the curve  $C_1$ . This may have something to do with the fact that it has been found best not to allow the iron of transformers to be magnetized beyond the saturation-point\*.

18. It is usual to assume that the electromotive force given by alternating-current dynamos can be represented by a sine function of the time with sufficient accuracy for practical purposes. In the case of a Ferranti dynamo at the Central Institution, however, the approximation to a sine wave is not very good.

The field-magnets were excited with a small current by means of accumulators. A resistance of 100 ohms was placed in the circuit to reduce the time-constant when the

\* See Mr. Gisbert Kapp's paper "On Alternate Current-Transformers," Proc. Soc. Tel. Engineers, Feb. 1888.



current was made. The armature was used as a test-coil, and was coupled in series with a D'Arsonval galvanometer and suitable resistances. A pointer was attached to the spindle, and a scale to the framework of the machine, so that the phase of the armature could be accurately read. The test swings were taken both on making and on breaking the exciting current, and were found practically identical. The swings were taken for many different phases of the armature for one complete alternation. The results showed that the ordinates of the curve of electromotive force gradually differed from those of the most favourably drawn sine curve to the extent of  $\pm 5$  per cent.

The values of the maximum ordinates of the E.M.F. curve were also compared for one complete revolution of the armature. Five swings were taken in the neighbourhood of each maximum in order to determine its amount. The curious result came out that these maxima varied alternately between values denoted by the numbers 302 and 333. The eight positive maxima did not vary more than  $\frac{1}{3}$  per cent. from the value 333, nor did the eight negative maxima vary more than this from the value 302. Since the area of the negative part of the curve must apparently be equal to that of the positive part, it follows that the curve of E.M.F. must be distinctly different from a sine curve. The result found in this case was not at all that which was looked for. It seemed probable that the values of the different maxima would vary gradually, and only repeat themselves after a complete revolution of the machine. It was expected that the curve of E.M.F. would be of the nature of a compound sine curve, having as one term a sine of large amplitude and a period corresponding with the current alternations, and having as a second term a sine of small amplitude but long period corresponding with the revolutions of the machine. Professor Perry has pointed out that, if the wave of E.M.F. is a sine curve marked with minor ripples caused by the presence of subsidiary sine waves of smaller period, the effect of induction in the circuit is to flatten out the minor ripples so that the current produced is more nearly a true sine wave than the electromotive-force wave which is producing it. This is, however, only true on the assumption that the minor ripples have the shorter period. It is very possible that in some alternating-current dynamos there is, due to some want of symmetry, a ripple of small amplitude but having a long period corresponding with the revolutions of the dynamo. Whenever this occurs, the action of induction is to magnify the minor ripples in comparison with the main alternating

wave, and the consequent current curve will, especially at high speeds, differ in a very marked degree from the E.M.F. curve of the machine. If the self-induction is a variable quantity, the current curves obtained will be still more rippled, and these ripples will be present at whatever speed the dynamo is revolved.

The well-known pulsations to which electric lights fed with alternating currents are subject seem to suggest that the currents obtained from the dynamos used are much less like sine waves than is generally assumed to be the case. These pulsations are so marked in the case of one important London installation that any quickly moving object, such as a walking-stick swayed rapidly to and fro presents, not a blurred image, as it would do under a continuous light, but several distinctly separate images, as if the light were at times very dim. The pulsations are too slow to be due to the ordinary alternations of the current, but might possibly be in time with the revolutions of the dynamo.

### *Condenser Discharges.*

19. It does not appear to have been noticed that self-induction, although always delaying the rise or fall of currents, may sometimes hasten the discharge of a condenser. The current which discharges a condenser has both to rise and fall. Self-induction in the discharge-circuit delays the rise of current less than the fall, because the potential-difference of the condenser is high when the current increases and low when it diminishes; the rate of change of current depends, not only on the coefficient of self-induction, but also on the E.M.F. tending to cause the change. The well-known oscillations produced when the self-induction is large are simply due to the fact that the current is kept flowing too long. The general result is that the time of discharge is lessened if the self-induction is not too great.

If a condenser of capacity  $K$ , charged to a potential  $V_0$ , be discharged by a wire having a resistance  $R$  and coefficient of self-induction  $L$ , the potential of the condenser at any time  $t$  is given by one of the equations

$$V = \frac{V_0}{T_1 - T_2} \{ T_1 e^{-\frac{t}{T_1}} - T_2 e^{-\frac{t}{T_2}} \},$$

$$V = V_0 e^{-\frac{Rt}{2L}} \frac{\sin(\beta t + \gamma)}{\sin \gamma},$$

according respectively as  $L$  is less or greater than  $\frac{KR^2}{4}$ .

In these equations  $T_1$  and  $T_2$  are the two quantities whose sum is  $KR$  and whose product is  $KL$ ;  $T_1$  is the larger of the two, but is less than  $KR$ .

The values of  $\beta$  and  $\gamma$  are given by the equations

$$\beta^2 = \frac{1}{KL} - \frac{R^2}{4L^2}, \quad \tan \gamma = \frac{2L\beta}{R}.$$

The time taken to discharge is proportional to  $T_1$  or to  $\frac{2L}{R}$ , according as  $L$  is less or greater than  $\frac{1}{4}KR^2$ . Both these values are less than  $KR$ , provided  $L < \frac{1}{2}KR^2$ ; so that the insertion of self-induction up to this value only hastens the discharge. The time will be a minimum when  $L = \frac{1}{4}KR^2$ ; and in this case  $T_1 = \frac{1}{2}KR$ . It is therefore possible to halve the time of discharge of a condenser by using a suitable amount of self-induction in the discharge-circuit.

A lightning discharge is essentially the same as that of a condenser, and it therefore seems likely that a small amount of self-induction in a lightning-conductor will only improve it. The value of  $K$  may be small, but that of  $R$  is large, since it quite possibly includes the resistance of the air-gap between the lightning-conductor and the charged cloud; so that the product  $KR^2$ , which determines the allowable value of  $L$ , may be quite comparable with the self-induction of iron lightning-rods, and may indeed far exceed it.

Quite recently Dr. Oliver Lodge has shown experimentally that an iron conductor is better than a copper one for high-tension electric discharges, and has suggested that, owing to magnetic lag, iron may have less self-induction for such discharges, than in cases where time is an unimportant factor. The foregoing considerations offer quite a different explanation. Further experiments will no doubt be necessary to settle the question; but in the meanwhile there seems to be no real basis for the idea that self-induction in a lightning-conductor is necessarily a disadvantage.

The writer, in conclusion, desires to thank Professor Ayrton for many valuable suggestions received while the experiments were being carried out.

LVIII. *The Efficiency of Incandescent Lamps with Direct and Alternating Currents.* By W. E. AYRTON and JOHN PERRY\*.

IT is now well understood that in order to economically distribute power by means of electricity it is necessary to employ a high potential-difference, or P.D., between the mains and a small current flowing through them, while considerations of safety require that the P.D. between the leads in the houses shall not exceed 100 or 200 volts. Hence some system of converting a large P.D. and small current into a small P.D. and large current has to be employed; and the four systems of conversion that have hitherto been devised consist in the employment of:—1. Motor-Dynamos, 2. Accumulators, 3. Alternating Current-Transformers, 4. Direct Current-Transformers.

Of these four methods the third is the one that is most extensively utilized at the present time; indeed it is the only system of conversion that is at all extensively used, at any rate in this country. But in view of some direct current system of conversion also coming into common use, there arises a question of considerable importance to the consumer, viz. Is more light obtained with the same expenditure of power with direct or with alternating currents? And apart from considerations of the electric distribution of power on a large scale, the answer to this question is of importance in supplying one factor towards the decision as to the relative advantages and disadvantages of direct and alternating currents for detached installations.

Where the electric energy supplied to a consumer has been charged by meter, people have, as a rule, been content to measure only the number of coulombs supplied, ignoring altogether any variation in the volts; but as electricity *per se* apart from the P.D. is of no commercial value whatever, and therefore is unlike water apart from pressure, it is clear that in estimating the value of a supply of electric energy for lighting purposes we must measure the watts and not merely the current. The problem, therefore, that we have attempted to solve is whether a Board of Trade unit (1000 watt-hours) is more valuable for lighting by incandescent lamps when the current is direct or when the current is alternating.

For a complete solution of this problem we must ascertain not merely the "efficiency of the lamp" or the candles per

\* Communicated by the Physical Society: read February 25, 1888.



watt with the two systems of supply, but also the life of the lamp, since the cost of lamp renewals may be as important a question for the consumer as the bill for electric power. Unfortunately, however, there does not exist, as far as we are aware, any accurate information as to the life of an incandescent lamp with various alternating P.Ds.; indeed the data at our disposal for the life of a lamp with various non-alternating P.Ds. is at most meagre. And if any Member of the Society can supply us with information regarding the life of some fixed type of incandescent lamp either with different direct P.Ds. or with different alternating P.Ds. we shall be grateful for the information, as it will furnish us with a further opportunity of using the method described in our paper read before this Society for deciding "On the Most Economical Potential Difference to Employ with Incandescent Lamps."\*

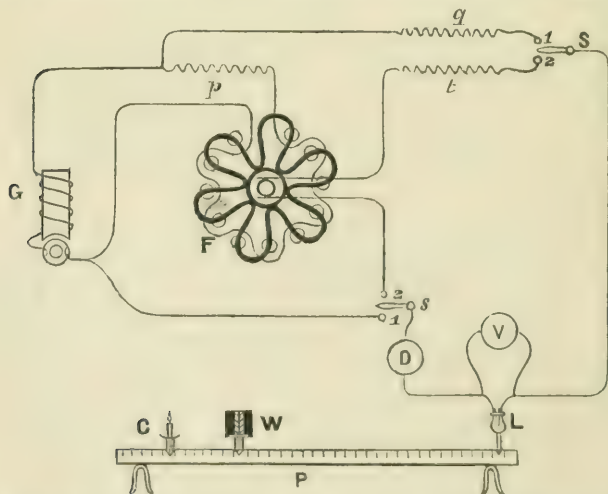
For the present, therefore, we shall confine ourselves solely to the question of efficiency, and as it is known that the efficiency of an incandescent lamp increases with the current passing through the lamp, it is clear that to obtain an accurate comparison of the efficiencies with direct and alternating currents, we must employ exactly the same current in both cases, or rather the same mean square of the current, for this will be most likely to develop the same rate of production of heat in the lamp, since this rate is proportional to the resistance into the mean square of the current, whether the current be direct or alternating, and whether or not there be self-induction in the circuit in which the heat is developed. We say will be most likely to develop the same rate of production of heat, since we must not assume without proof that for the same mean square of current the resistance of a carbon filament is the same whether the current be direct or alternating.

Some writers have stated that the current, as measured by an electro-dynamometer, required to be sent through an incandescent lamp when emitting a definite amount of light, had a different value when the current was an alternating one from what it had when the current was direct. This difference might be due either to a defect in the dynamometer measurement or to some variation in the light standard. If the wire with which a dynamometer is wound be thick, then the current-density may be far from uniform when the current is alternating; and on this account, as observed by Captain Cardew some years ago, a dynamometer might give different readings for different rates of alternations of the current,

\* *Phil. Mag.* April 1885.

while an incandescent lamp in circuit with it remained equally bright. To avoid this possible cause of error the dynamometer, which was constructed by Messrs. Shepherd, Vignoles, and Wheatley, the three of the students of the Central Institution who carried out the investigation, was wound with much finer wire than would usually be employed in the construction of a dynamometer not required to read currents much below one ampere. The dynamometer was for that reason unnecessarily sensitive, and it required a fairly strong spring to control the motion of the suspended coil. This led to an unnecessary waste of energy in the dynamometer, but that was of no consequence in this investigation, as our object was to measure the mean square of the current most accurately, and not to satisfy the condition, which is of considerable importance in the design of commercial measuring-instruments, of wasting as little energy as possible in the instruments.

To the suspended coil of the dynamometer was attached a mirror, and the values of the deflections of the spot of light were determined by direct comparison with the simultaneous readings of an accurately calibrated magnifying spring-ammeter when various direct currents were successively sent through the circuit. The sensibility was such that a deflection of 400 scale-divisions was produced on a scale 68·88 inches away, for a current of 2·53 amperes, which corresponds with a deflection of 140 scale-divisions for 1·5 ampere,



and this was about the usual current passing through the dynamometer in the actual lamp experiments. The value

of the current could, therefore, be accurately measured by this dynamometer, which is indicated by D in the figure, and by knowing what fraction of the current passing through the dynamometer D passed through the non-inductive voltmeter V, the current passing through the lamp is known.

Any error that might have arisen from a variation of the light-standard was eliminated by taking successive readings with a direct current produced by a Gramme-dynamo G, and with an alternating current produced by a Ferranti dynamo F, the switches S and s being turned to 1, 1 in the first case and to 2, 2 in the second. The Gramme-dynamo G was also used to excite the field magnets of the Ferranti, a suitable current being obtained by a proper adjustment of the resistance  $p$ . By means of the resistances  $q$  and  $t$  the direct and alternating currents passing through the incandescent lamp L could be respectively varied; and it was found that if these resistances were so adjusted that the reading of the dynamometer D was the same in both cases, so also was the reading of the non-inductive voltmeter V.

It is known when power is supplied by means of an alternating current to a circuit of resistance  $r$  ohms and coefficient of self-induction  $l$  secohms that

$$\frac{\text{number of true watts}}{\text{number of measured watts}} = \frac{r\tau}{\sqrt{l^2\pi^2 + r^2\tau^2}},$$

where the measured watts are obtained by multiplying  $\sqrt{A^2}$ , the square root of the mean square of the amperes as measured by the dynamometer, by  $\sqrt{V^2}$ , the square root of the mean square of the volts as measured by the non-inductive voltmeter, and where  $\tau$  is the time between one alternation and the next, or half a complete period. Therefore the

$$\text{number of true watts} = \frac{r\tau\sqrt{A^2V^2}}{\sqrt{l^2\pi^2 + r^2\tau^2}},$$

where in our case  $r$  and  $l$  are the resistance and coefficient of self-induction of the carbon filament of the lamp. In the first set of experiments a lamp with a looped filament was employed; but the experiments seemed to indicate that the value of  $l$  was not quite small enough to make the term  $l^2\pi^2$  absolutely negligible, or else that there was some slight mutual induction between the dynamometer-coils and a small brass vessel containing oil, in which moved a damping vane attached to the moving coil. The results were therefore discarded, and this vessel was replaced by one made of non-conducting material, and all metal near the dynamometer

was as far as possible removed. The lamps subsequently employed were first one with a M-shaped filament, and another with a simple horseshoe-shaped filament, each possessing little self-induction.

The light was measured by comparison with a standard candle, the two being placed 130 centimetres apart on the photometer P, and a screen composed of two pieces of paraffin-wax W, with silvered paper between them, was adjusted until the two pieces of wax appeared equally bright, the comparison being first made when the screen was looked at through ruby-red glass and then through signal-green glass.

The following is a sample of the results obtained, *a* and *b* being the distances respectively of the screen from the incandescent lamp and from the standard candle.

TABLE I

	$\sqrt{A^2}$	$\sqrt{V^2}$	Watts.	Green.			
				<i>a.</i>	<i>b.</i>	Candles.	Watts per Candle.
Ferranti.....	1.34	50	67	107.7	22.3	23.33	2.872
Gramme.....	1.34	50	67	105.8	22.2	23.52	2.848
Ferranti.....	1.34	50	67	107.9	22.1	23.74	2.823
Gramme.....	1.34	50	67	108	22	24.11	2.779
	$\sqrt{A^2}$	$\sqrt{V^2}$	Watts.	Red.			
				<i>a.</i>	<i>b.</i>	Candles.	Watts per Candle.
Ferranti.....	1.34	50	67	106.5	23.5	20.52	3.265
Gramme.....	1.34	50	67	106.4	23.6	20.34	3.295
Ferranti.....	1.34	50	67	106	24	19.51	3.435
Gramme.....	1.34	50	67	105.9	24.1	19.32	3.469

It will be observed that in the set of four successive observations given in Table I. for green light, the candle-power of the lamp appears to be steadily increasing, a result probably due to the brightness of the candle slowly diminishing; but although this would make the *absolute* determination of the efficiency of the incandescent lamp inexact, it introduces no error in the determination of the *relative* efficiencies of the lamp with direct and alternating currents, since every observation with



the direct current was immediately followed by one with alternating current, so that the means of all the corresponding direct-current measurements may be safely compared with the means of all the alternating-current measurements. Similarly for this investigation it is unimportant whether the standard candle used on one day was slightly more or less bright than the standard candle used on the following day. Further, since for any two successive observations with the same coloured light with direct and with alternating current the screen was at practically the same distance from the lamp, the fact that the rays of light coming from the lamp made a somewhat different angle with the surface of the paraffin-wax from the angle made by the rays coming from the candle, introduced no error in this investigation of *comparative* efficiencies, although it might very likely do so in an absolute determination of the efficiency of an incandescent lamp. In fact the simple device of successively taking observations with direct and with alternating currents throughout the whole investigation removed most of the objections that usually may be made against photometric determinations carried out with a candle as the standard of light.

Disregarding the experiments made with the lamp with the looped filament, for the reason given above, Table II. gives the summary of the results obtained; and the conclusion to be

TABLE II.—Mean Values.

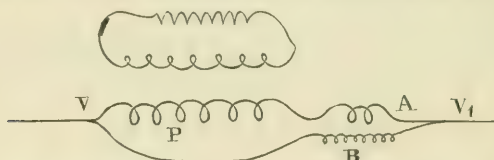
Lamp.	No. of Experiments made.	$\sqrt{A^2}$ .	$\sqrt{V^2}$	Watts per Candle. White Light.				Time of an alternation, in seconds.
50 volt M-shaped filament. }	20	1.25	50.5	Gramme. 3.053		Ferranti. 3.033		$\frac{1}{453}$
				Green Light. Gramme. Ferranti.		Red Light. Gramme. Ferranti.		
50 volt horseshoe-shaped filament. }	19	1.30	50.0	2.597	2.534	3.100	3.100	$\frac{1}{453}$
50 volt horseshoe-shaped filament. }	20	1.34	50.0	2.935	2.966	3.254	3.164	$\frac{1}{167}$
50 volt horseshoe-shaped filament }	16	1.35	50.0	2.900	3.073	3.504	3.477	
Mean of the last three results .....				2.811	2.857	3.286	3.247	
Mean of all the 75 experiments .....				Gramme. 3.0490		Ferranti. 3.0497		

In all the experiments the plane of the filament was perpendicular to the screen.

drawn from them is that although the watts per candle for green light are not quite the same for direct and for alternating currents, and although the volts per candle for red light are also not exactly the same for direct and alternating, still the difference between the results for the same colour is so small that it may be put down to experimental errors; and this, combined with the fact that the mean of all the 75 experiments gives practically the same number of watts per candle for both direct and alternating currents, leads to the practical certainty that *the efficiency of an incandescent lamp is the same for both direct and alternating currents.*

LIX. *On the Measurement of the Power supplied to the Primary Coil of a Transformer.* By E. C. RIMINGTON\*.

IN the discussion on Mr. Kapp's paper on Transformers, at the Society of Telegraph Engineers, Professor Ayrton gave a formula for calculating the true power supplied to the primary from the reading of a Siemens wattmeter. The thick wire coil of the wattmeter is in series with the primary



coil, and the fine wire coil connected as a shunt on the two, as in the diagram, where P is the primary coil, A the thick and B the fine wire coil of the wattmeter. Let  $r_1$  be the resistance of the primary P including A, and  $L_1$  its coefficient of self-induction also including A; let  $r_2$  and  $L_2$  be the resistance and coefficient of self-induction of B;  $i_1$  and  $i_2$  the currents in P and B respectively. Let  $e$  be the potential-difference between the points V and  $V_1$ . Now

$$e = E \sin at, \text{ where } a = \frac{2\pi}{T},$$

T being the periodic time, and E = the maximum value of  $e$ . Also

$$i_1 = A_1 \sin (at - \psi_1), \text{ where } \tan \psi_1 = \frac{aL_1}{r_1},$$

and

$$i_2 = A_2 \sin (at - \psi_2), \text{ where } \tan \psi_2 = \frac{aL_2}{r_2}.$$

\* Communicated by the Physical Society: read March 10, 1888.

Let  $\delta$  be the reading on the torsion-head ; then

$$\begin{aligned}\delta &= \frac{k}{T} \int_0^T i_1 i_2 dt ; k \text{ being some constant.} \\ \delta &= \frac{k}{T} A_1 A_2 \int_0^T \sin (at - \psi_1) \sin (at - \psi_2) dt, \\ &= \frac{k A_1 A_2}{2T} \int_0^T \{ \cos (\psi_2 - \psi_1) - \cos (2at - \psi_1 - \psi_2) \} dt, \\ &= \frac{k A_1 A_2}{2} \cos (\psi_1 - \psi_2).\end{aligned}$$

Now  $A_2 = \frac{E}{S}$ , where  $S = \sqrt{r_2^2 + a^2 L_2^2}$ .

Hence

$$\delta = \frac{k}{S} \cdot \frac{E A_1}{2} \cos (\psi_1 - \psi_2).$$

Now mean power, or

$$\begin{aligned}p_m &= \frac{1}{T} \int_0^T e i_1 dt, \\ &= \frac{1}{T} E A_1 \int_0^T \sin at \cdot \sin (at - \psi_1) dt \\ &= \frac{E A_1}{2} \cos \psi_1.\end{aligned}$$

Therefore

$$p_m = \frac{\delta}{k} \cdot S \cdot \frac{\cos \psi_1}{\cos (\psi_1 - \psi_2)}.$$

But

$$\begin{aligned}\frac{\cos \psi_1}{\cos (\psi_1 - \psi_2)} &= \frac{\cos \psi_1}{\cos \psi_1 \cos \psi_2 + \sin \psi_1 \sin \psi_2} \\ &= \frac{1}{\frac{\cos \psi_2}{1 + \tan \psi_1 \tan \psi_2}}, \\ \frac{\cos \psi_1}{\cos (\psi_1 - \psi_2)} &= \frac{\sqrt{1 + \tan^2 \psi_2}}{1 + \tan \psi_1 \tan \psi_2} = \frac{\sqrt{1 + \frac{a^2 L_2^2}{r_2^2}}}{1 + \frac{a^2 L_1 L_2}{r_1 r_2}} \\ &= \frac{r_1 S}{r_1 r_2 + a^2 L_1 L_2}.\end{aligned}$$

Hence

$$m = \frac{\delta}{k} \frac{r_1 S^2}{r_1 r_2 + a^2 L_1 L_2}.$$

Now for permanent currents,

$$\delta = k \frac{e}{r_2} C, \text{ and } eC = K\delta,$$

where  $K$  is the constant of the instrument for watts.

Therefore

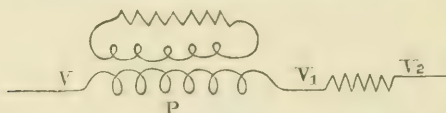
$$k = \frac{r_2}{K},$$

or

$$p_m = K\delta \cdot \frac{r_1(r_2^2 + a^2 L_2^2)}{r_2(r_1 r_2 + a^2 L_1 L_2)}.$$

In order to be able to make use of this result it is necessary to know the coefficients of self-induction of  $P + A$  and  $B$ ; the latter may be found once for all by any of the well-known methods and its value marked on the instrument; but the former will require to be determined for the same values of the currents in the primary and secondary coils of the transformer as are flowing when the power is measured, since the apparent coefficient of self-induction of the primary coil depends on the saturation of the iron of the transformer and also on the current in the secondary coil. The best method of measuring  $L_1$  under these conditions is that due to Joubert.

Connect an inductionless resistance  $R$  in series with the



primary, and pass an alternating current of known period through the two; arrange the resistances of the primary and secondary circuits so that the currents in them have about the same values as when the power-measurement was made. Now connect a high-resistance Siemens electro-dynamometer\* between  $V$  and  $V_1$ , and let the reading be  $\delta_1$ ; again connect it between  $V_1$  and  $V_2$  and let the reading be  $\delta_2$ .

Then

$$\frac{\sqrt{r_1^2 + a^2 L_1^2}}{R} = \sqrt{\frac{\delta_1}{\delta_2}},$$

$$L_1 = \frac{1}{a} \sqrt{R^2 \frac{\delta_1}{\delta_2} - r_1^2}.$$

\* A Cardew voltmeter may be employed, in which case

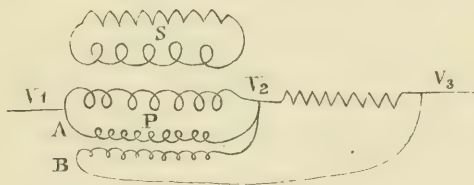
$$L_1 = \frac{1}{a} \sqrt{R^2 \frac{\delta_1^2}{\delta_2^2} - r_1^2}.$$



This method of calculating the mean power supplied to the primary of a transformer is, however, inconvenient, as it entails the use of a high-resistance dynamometer, with which two observations must be taken to obtain  $L_1$ , in addition to the trouble of adjusting the resistances of the two circuits to obtain the same conditions as when the wattmeter-reading was taken.

The following method, in which a high-resistance electro-dynamometer is employed, enables the power given to the primary coil to be measured from one reading without the knowledge of either the resistance or the coefficient of self-induction of the primary.

Let the two coils of the electro-dynamometer be A and B, and let their resistances and coefficients of self-induction be  $r_1 r_2$  and  $l_1 l_2$  respectively; moreover, let  $\frac{l_1}{r_1} = \frac{l_2}{r_2}$ , that is to say, let the time-constants of the two coils be the same; this can be easily effected by putting an inductionless resistance in series with one or other of the coils. Connect as in the diagram.



Let the primary coil be put in series with an inductionless resistance  $R$ . Let the potential-difference between  $V_1$  and  $V_2 = E_1 \sin at$ ; then  $i$ , the current through the primary coil and through  $R = \frac{E_2}{R} \sin (at - \psi)$ , where  $E_2$  is the maximum potential-difference between  $V_2$  and  $V_3$ .

Let  $\psi_1$  and  $\psi_2$  be the angles of lag of the coils A and B respectively, and let  $i_1$  and  $i_2$  be the currents through them at some instant  $t$ .

Then, if  $\delta$  be the reading of the torsion-head,

$$\begin{aligned} \delta &= \frac{k}{T} \int_0^T i_1 i_2 dt, \\ &= \frac{k}{T} \cdot \frac{E_1 E_2}{S_1 S_2} \int_0^T \sin (at - \psi_1) \sin (at - \psi - \psi_2) dt, \\ &= \frac{k}{2} \cdot \frac{E_1 E_2}{S_1 S_2} \cos (\psi_1 - \psi_2 - \psi); \end{aligned}$$

where

$$S_1 = \sqrt{r_1^2 + a^2 l_1^2} \quad \text{and} \quad S_2 = \sqrt{r_2^2 + a^2 l_2^2}.$$

Now

$$\tan \psi_1 = \frac{al_1}{r_1}, \quad \text{and} \quad \tan \psi_2 = \frac{al_2}{r_2}.$$

Hence

$$\tan \psi_1 = \tan \psi_2, \quad \text{since} \quad \frac{l_1}{r_1} = \frac{l_2}{r_2},$$

or

$$\psi_1 - \psi_2 = 0.$$

Therefore

$$\delta = \frac{k}{2} \cdot \frac{E_1 E_2}{S_1 S_2} \cos \psi.$$

Now mean power given to the primary coil, or

$$\begin{aligned} p_m &= \frac{E_1 E_2}{2R} \cos \psi, \\ &= \frac{\delta}{k} \cdot \frac{S_1 S_2}{R}. \end{aligned}$$

For permanent currents,

$$\delta = k \frac{E_1}{r_1} \cdot \frac{E_2}{r_2} = k E_1 C \cdot \frac{R}{r_1 r_2}.$$

Also

$$E_1 C = \frac{K}{R} \delta,$$

where  $\frac{K}{R}$  is the constant for watts.

Hence

$$k = \frac{r_1 r_2}{K}.$$

Therefore

$$p_m = \frac{K}{R} \cdot \delta \cdot \frac{S_1 S_2}{r_1 r_2}.$$

$S_1 S_2$  may be written

$$r_1 r_2 \sqrt{\left(1 + \frac{a^2 l_1^2}{r_1^2}\right) \left(1 + \frac{a^2 l_2^2}{r_2^2}\right)} = r_1 r_2 (1 + \tan^2 \psi_1).$$

Hence

$$p_m = \frac{K}{R} \cdot \delta \cdot (1 + \tan^2 \psi_1).$$

In order that the non-inductive resistance  $R$  shall not absorb too much power,  $B$  should be the movable coil of the electro-dynamometer, as this is generally the one of lower resistance;  $\frac{E_1}{S_1}$  should have about the same value as  $\frac{E_2}{S_2}$ , or

$$\frac{E_2}{S_2} = \frac{E_1}{S_1} \quad \text{approximately.}$$

Now

$$\frac{S_2}{S_1} = \frac{r_2 \sqrt{1 + \tan^2 \psi_2}}{r_1 \sqrt{1 + \tan^2 \psi_1}} = \frac{r_2}{r_1};$$

that is, if  $r_2$  is considerably lower than  $r_1$ ,  $E_2$  will be in the same proportion lower than  $E_1$ , and hence  $R$  may be a good deal smaller than the impedance of the primary.

LX. *On the Polarization of Platinum Plates.*

*By C. H. DRAPER, B.A., D.Sc.\**

THE fact of the decomposition of dilute sulphuric acid by the passage between platinum electrodes of electricity through a voltameter containing it, involves an expenditure of energy. This energy is made available by a sudden fall of the current through a certain difference of potential numerically equal to the energy absorbed by the quantity of water which one unit of electricity decomposes. The cause of this sudden fall of potential within the voltameter over and above that due to the resistance of the liquid as a conductor is the modified condition assumed by the platinum plates, which leads to the phenomenon called polarization. Those portions of the gaseous products which come first into contact with the platinum, especially in the case of hydrogen, either form with the platinum a chemical combination, or undergo such physical or chemical modification by occlusion or condensation as results in a loss of energy and resulting fall of potential. This modification of condition progressively diminishes in the successive layers as we proceed outwards from the platinum plate, until a layer is reached which is beyond the reach of the influence of the platinum, and where the gas escapes freely as fast as it is formed. The result of this polarized condition of the electrodes is manifested as an electromotive force opposed to that which produces the current. The electromotive force of polarization has for each electrolyte a theoretically fixed maximum value, and experimentally there appears to be an approximately constant maximum value which is always above the theoretical value when any considerable current is passing, and which varies with the conditions of the experiment. Thus Professor Tait, in some experiments described in the 'Philosophical Magazine' for September 1869, found (taking the electromotive

\* Communicated by the Author.

force of a Daniell cell as 1.1 volt), using

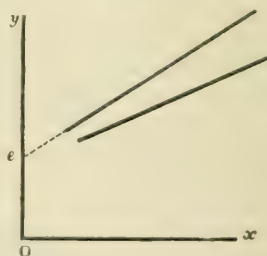
2 Grove cells, polarization E.M.F. = 2.15 volts.

4       "       "       "       = 2.3       "

8       "       "       "       = 2.5       "

The objects with which the experiments described below were undertaken were to ascertain whether the electromotive force of polarization has a sensibly constant value beyond a certain maximum limit, and what is the effect upon it of change of temperature. The method of experimenting was to pass currents of various known strengths through a voltmeter filled with dilute sulphuric acid, and to note the difference of potential between the electrodes by means of a quadrant-electrometer. Since Ohm's law is true for electrolytes, at least within the limits of probable error in experiments of this kind, there will be a difference of potential between the electrodes just as between the two ends of a wire. If  $R$  be the resistance of the column of liquid, and  $C$  a value of the current which is not less than that value corresponding to  $e$  the maximum value of the electromotive force of polarization, and if  $E$  be the difference of potential observed between the electrodes, then  $E = e + CR$ . Any increase in the value of  $C$  will now be the accompaniment of an increase in the value of  $E$ . If, then, the observed values of  $E$  and  $C$  be marked out along axes at right angles to each other, the equation  $E = e + CR$  will represent a straight line,  $e$  being represented by the intercept on the axis of potential ( $Oy$ ), and the resistance of the voltmeter being indicated by the tangent of the angle which the straight line makes with the axis of the current ( $Ox$ ). If, then, the temperature at which the process takes place has an effect on the amount of the maximum polarization, this will be manifested by a change in the value of the intercept, while the change in the slope of the line will indicate the change in the resistance of the voltmeter.

Fig. 1.

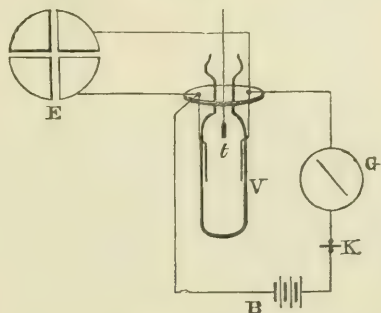


*Apparatus.*—The circuit consisted of a battery,  $B$ , and a voltmeter,  $V$ , connected through a commutating-key,  $K$ , with a tangent-galvanometer,  $G$ , the electrodes of the voltmeter being connected also with the terminals of a quadrant-electrometer,  $E$ . The voltmeter was placed inside a large



water-bath, and a thermometer,  $t$ , was suspended dipping into the liquid of the voltameter. A battery of Grove cells was used, the number of cells varying from 1 to 10. The voltameter was a glass tube of an inch in diameter, and six inches high. The electrodes were strips of platinum about  $1\frac{1}{2}$  inch long and nearly  $\frac{1}{4}$  inch wide, connected by pieces of platinum wire which passed through the glass to binding-

Fig. 2.



screws fixed on a wooden collar which surrounded the neck of the instrument. The water-bath was contained in a copper vessel, and heated by a Bunsen burner.

The electrolytic liquid consisted of a 10-per-cent. solution of pure sulphuric acid (sp. gr. 1.842) in distilled water. The voltameter was emptied and refilled at the commencement of each day's experiments, so that the solution never varied much in strength. The thermometer employed was one whose error had been previously found to be negligibly small; and it being necessary that the temperature of the liquid in the voltameter should be accurately known, it was suspended with its bulb dipping into the liquid. Some experiments were made to see whether error was likely to be introduced by this; and it was found that when the stem of the thermometer was actually between the electrodes, it had a small but appreciable effect on the resistance of the column of liquid, but when the bottom of the bulb was above the top of the plates there was no measurable effect. It was concluded that the thermometer might be placed with its bulb dipping into the liquid but above the platinum plates, and that any small alteration in its position from day to day would not introduce sensible error. The voltameter was always filled quite full, so that the introduction of the thermometer did not alter the resistance by raising the level of the liquid.

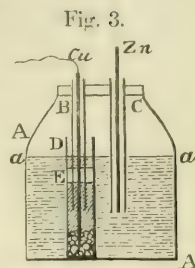
*Preliminary Experiments.*—A set of readings was first taken with the temperature approximately constant at  $18^{\circ}.5\text{ C.}$ , and with rather weak currents, the tangent-galvanometer having two rings and sixty turns of wire. The readings were:—

Electrometer-divisions.	Tangent of galvanometer-deflection.
26.5 . . . . .	.161
27.3 . . . . .	.260
28.6 . . . . .	.471
29.5 . . . . .	.922
30.0 . . . . .	1.340

Here evidently the current was increasing faster than the potential-difference; and the inference was that the polarization was increasing in amount, and did not reach a steady value with such currents as could be measured by this galvanometer. A galvanometer with a single ring was therefore used in the greater part of the experiments. To obtain a deflection as high as  $45^{\circ}$  on this galvanometer it was necessary to use eight or ten Grove cells; and the potential-difference with such strong currents was such as to send the spot of light off the scale when the distance from mirror to scale was that for which the divisions represented equal differences of potential. The scale was therefore moved nearer the mirror and calibrated. A current was sent by five Leclanché cells through 12,000 ohms resistance, while the electrometer was joined to two movable points in this resistance. Between these two movable points various resistances were included, from 1000 to 12,000 ohms; the readings being taken up one side of the scale and back, then up the other side and back to zero, in order to eliminate the effect of loss of charge in the electrometer. The scale was thus divided into twenty-four parts, corresponding to equal differences of potential, and a table was drawn up from which the value of any deflection could be obtained in terms of the central scale-division. The scale was put as accurately as possible at the same distance from the mirror on each day; but in order to ensure absence of error from displacement of the scale, as well as to have a less arbitrary unit than a scale-division, the deflection produced by a standard cell was taken at the beginning and end of each set of observations.

*The Standard Cell.*—The cell chosen was a modified form of the Daniell, resembling that described by Professor Lodge in the *Philosophical Magazine* for January 1878. In a

small glass bottle, A A, was placed a weak solution of zinc sulphate, which reached to the level, *aa*. The bottle was closed by a cork, through which passed two glass tubes, B, C, of about  $\frac{1}{2}$ -inch bore, and open at each end. C contained a strip of amalgamated zinc, which could be lowered into the zinc sulphate or raised out of it as required. The tube B contained a thick copper wire, which was gutta-percha covered (except for about half an inch where it passed beyond the lower end of the tube), and passed into a test-tube, D, through a cork, E. This test-tube was filled up to the level *aa* with a saturated copper sulphate solution, and the naked part of the copper wire was embedded among crystals of copper sulphate placed at the bottom of the test-tube. The level of the liquids was about half an inch below the top of the test-tube. Scarcely any diffusion of the liquids is possible; and this form of the Daniell, though useless as a means of producing currents, has a very constant electromotive force.



*Readings.*—A series of experiments was made at the temperatures  $40^{\circ}$ ,  $60^{\circ}$ ,  $80^{\circ}$  C., and with the water in the bath boiling. The readings were taken in sets, proceeding from weaker to stronger currents and back again, so as to eliminate the effects of possible leakage of the electrometer. The galvanometer-deflections were corrected for torsion of the suspending fibre, and the electrometer-readings for scale-error. In the following table, E indicates the electrometer-readings and  $\theta$  the deflection of the galvanometer-needle.

Temp. $40^{\circ}$ .		Temp. $60^{\circ}$ .		Temp. $80^{\circ}$ .	
E.	Tan $\theta$ .	E.	Tan $\theta$ .	E.	Tan $\theta$ .
25.0	.1187	22.95	.1026	22.5	.1178
27.0	.1585	24.9	.1463	24.15	.1495
28.9	.2045	28.3	.2127	26.0	.1927
31.45	.2621	31.9	.2881	28.1	.2292
35.1	.3476	36.6	.3925	30.25	.2766
38.9	.4323	40.4	.4661	33.0	.3396
41.5	.5024	44.25	.5722	36.7	.4171
44.8	.5722	49.6	.6829	39.2	.4828
48.0	.6363			41.4	.5446
52.0	.7120			45.0	.6041
54.8	.7673			53.0	.7651
56.65	.7999			60.1	.8810

The readings taken with the liquid boiling were, as might be expected, somewhat irregular, but in all the potential value for the same current was considerably lower than at  $80^{\circ}$ .

It is evident, on comparing the values of  $E$  and  $\tan \theta$  at the different temperatures, that for any given value of the current the potential-difference diminishes with rise of temperature. It was then decided to reduce the results to absolute measure, so that something more than comparative values might be indicated.

*Reduction to Absolute Measure.* (1) *The Current.*—For this it was necessary to find the galvanometer-constant and the horizontal intensity of the earth's magnetism for the position of the galvanometer.

*Galvanometer-constant.*—The single ring had a mean diameter of 43.21 centim., and its calculated constant was thus .2908; as determined by the quantity of hydrogen liberated in the electrolysis of water it was .2902. The mean of these, .2905, was taken.

*Determination of H.*—The value of the horizontal component of the earth's magnetism for the position of the galvanometer was determined by means of a Kew unifilar magnetometer to be .1830.

(2) *The Electromotive Force.*—To reduce the electrometer-readings to absolute units the E.M.F. of the standard cell had to be ascertained, since the E.M.F. of a Daniell cell has been variously given from .978 volt (Latimer Clark) to 1.137 volt (Kohlrausch), depending on the strength of the solutions employed.

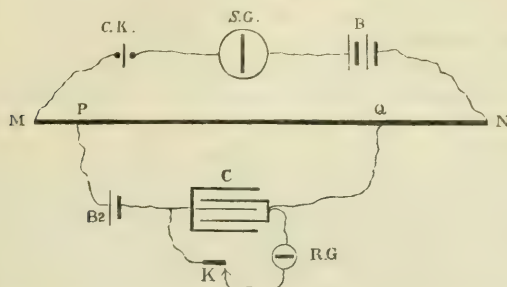
*Determination of the E.M.F. of the Standard Cell.*—The following method was adopted to find the E.M.F. The sensitive galvanometer employed in Poggendorff's method was replaced by a condenser and the galvanometer was placed as a shunt to the condenser.

The accompanying figure shows the connexions. B a battery of two Daniell cells of the gravitation-pattern is connected through C K, a commutating-key, with a circuit containing S G, a standard tangent-galvanometer, and M N, a resistance-box containing 12,000 ohms. B<sub>2</sub>, the standard cell, has one terminal joined to P, a movable plug in the resistance-box, and the other terminal to one coating of the condenser, C, of which the other coating is joined to Q, another movable plug. R G is a delicate reflecting-galvanometer joined, through a contact-key, K, with the condenser C. A steady current was allowed to flow round the circuit B M N, and the resistance P Q was so adjusted, without altering the whole resistance M N, that on depressing the key K there



was no deflection on the reflecting-galvanometer. The E.M.F. of  $B_2$  is then equal to the difference of potential between the points P, Q in the primary circuit.

Fig. 4.



The tangent-galvanometer had two equal parallel coils of approximately square section placed at a distance apart nearly equal to their mean radius, which was about 18.25 centim. Each coil consisted of 22 layers of 20 turns each. The deflections were read by a telescope placed underneath the scale, which was straight and was divided into centimetres and millimetres. The distance from mirror to scale was 345.44 centim. The galvanometer-constant  $\gamma$  was calculated to be 1.0004618, so that, neglecting torsion, the absolute strength of a current measured on this galvanometer is  $0.004618 H \tan \theta$ .

Torsion.—On turning the torsion-head through  $360^\circ$  the needle was deflected through  $118'$ . The factor needed to correct for this effect is therefore  $\frac{21600}{21482} = 1.0055$ .

Value of  $H$ .—The value of  $H$  for the position of the galvanometer was ascertained by comparing the time of vibration of the magnetometer-needle in that position with the time of vibration in the place for which the value of  $H$  had been accurately ascertained. The mean of two determinations of the period of vibration was 4.314 seconds. In the room where the value of  $H$  was .1830, the time of vibration was 4.2085 seconds; so that the value of  $H$  in this spot was  $.183 \times (4.2085)^2 \div (4.314)^2$ , whence  $H = .1742$ .

Readings.—The resistance in the main circuit was about 4000 ohms, and 10 microfarads was the capacity employed, and the arrangement was sensitive to within .25 per cent. of the quantity to be measured. Readings of the galvanometer were taken on both sides of zero, and the mean of four, which were very close together, gave

No. of ohms in main circuit.	No. of ohms between P.Q.	Deflection on scale in centim.
4115	2110	44·64

If  $\theta$  be deflection of needle,  $\tan 2\theta = 44\cdot64 \div 345\cdot44 = \cdot1292$ , whence  $2\theta = 7^\circ 21' 45''$  and  $\theta = 3^\circ 40' 52''$ . Corrected for torsion,  $\theta = 3^\circ 42' 5''$  and  $\tan \theta = \cdot06469$ ; whence if  $C$  = current in absolute measure,  $C = \cdot004618 \times \cdot1742 \times \cdot06469$ . The resistance between the points P, Q =  $2110 \times 10^9$  in absolute units; therefore E.M.F. of cell =  $CR = 2110 \times 10^9 \times \cdot004618 \times \cdot1742 \times \cdot06469 = 1\cdot098 \times 10^8$ , or  $1\cdot098$  volt.

Here and throughout the old value of the ohm is taken.

After the results were reduced another correction was applied for the effect of the platinum wires to the end of which the electrometer-wires were joined, and which, being in the battery-circuit, caused the potential-readings to be higher than would have been the case if the electrometer had been joined directly to the electrodes. Their resistance at  $16^\circ$ , as determined by the Wheatstone-bridge, was  $\cdot22$  ohm; and from Siemens' formula,

$$r_t = r_o \{ \cdot03969 T^{\frac{1}{2}} + \cdot002164 T - \cdot2413 \}.$$

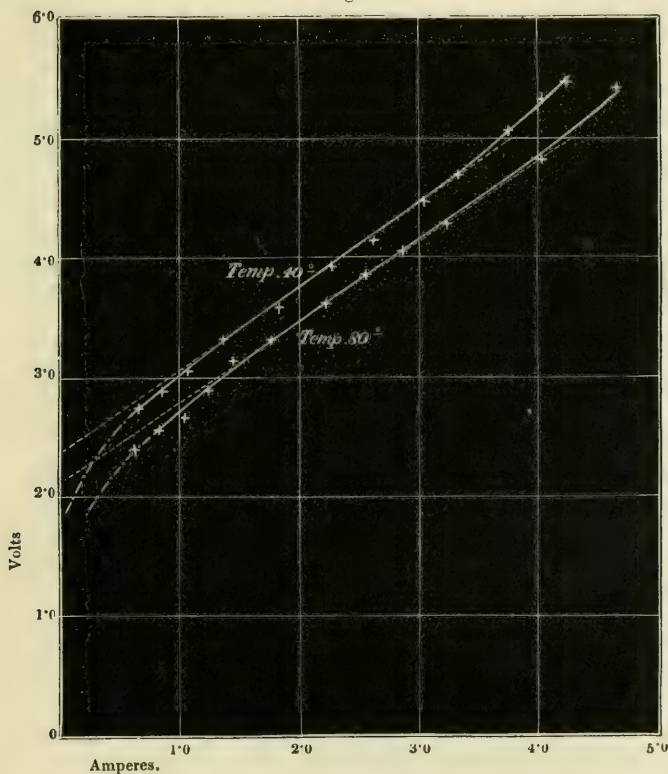
The resistance at  $40^\circ$  is thus  $\cdot253$  ohm, and at  $80^\circ$  is  $\cdot283$  ohm.

The following table gives the reduced observations for  $40^\circ$  and  $80^\circ$ , and the curve in the accompanying figure shows the relation between them. The observations for  $60^\circ$  and  $100^\circ$  are not given, as they would only serve to confuse the figure. The current-values are in amperes, and the differences of potential in volts.

Temperature $40^\circ$ C.		Temperature $80^\circ$ C.	
Current.	Difference of potential.	Current.	Difference of potential.
0·63	2·75	0·62	2·40
0·84	2·92	0·79	2·54
1·09	3·08	1·02	2·64
1·39	3·31	1·22	2·87
1·85	3·59	1·47	3·11
2·31	3·92	1·80	3·29
2·67	4·14	2·22	3·57
3·04	4·42	2·57	3·85
3·38	4·70	2·89	4·01
3·78	5·06	3·21	4·24
4·08	5·31	4·07	4·80
4·25	5·47	4·63	5·44

The characteristics of both lines are the same. At both temperatures, for values of the current lower than .5 ampere the line is concave to the axis of current. An accident to the voltmeter during the time that this part of the curve was being investigated rendered these readings less trustworthy than the rest. The course of the lines is therefore only roughly indicated by dots (—.—.—.—) in this part, but it may be noticed that they seem to tend towards a common point on the axis of potential at about 1.5 volt. The curved

Fig. 5.



part of the line extends to a point where the current-value was about 1.3 or 1.4 ampere, the current-strength required for the establishment of maximum polarization being somewhat greater at the higher temperature. Beyond this point both lines are for some distance straight, a deviation becoming again manifest when the current-value reaches 3.5 amperes, where the line turns up and becomes convex to the current-

axis. The explanation of this seems almost certainly to be an increase in the resistance of the liquid column, owing to the presence in considerable quantity of the gaseous products in the path of the current. A straight line dotted thus ..... has been ruled through those points which lie on the straight part of the curve, to cut the axis of potential. Representing this straight line by the equation  $E = e + CR$ , and putting in the numerical values of  $E$  and  $C$ , a series of equations was obtained containing  $e$  and  $R$ . The mean value of  $e$  at  $40^\circ$  is  $2.32$ , and at  $80^\circ$   $2.08$ ; and these values of the intercept on the axis of potential represent in volts the values of the maximum steady electromotive force of polarization at these temperatures under the conditions of the experiment. The difference  $.24$  volt, produced by a rise of  $40^\circ$  in temperature, gives  $.26$  per cent. as the rate of variation per degree at temperature  $40^\circ$ . The other constant  $R$  of the straight line, representing the tangent of its angle of inclination to the axis of current, being the quotient of potential by current, indicates the resistance of the voltmeter under the conditions of the experiment. The mean values of  $R$  obtained from the equations were for  $40^\circ$ ,  $.69$ , and for  $80^\circ$ ,  $.67$ ; these numbers representing in ohms the working resistance of the voltmeter.

*Conclusions.*—(1) The opposing electromotive force of polarization which arises in cells when at work depends on the value of the current passing through them when that current is below a certain value, increasing, but more and more slowly, with the current.

(2) There is a maximum value of the polarization regarded only as a function of the current-strength, beyond which any increase in the strength of the current has no effect upon it.

(3) The electromotive force of polarization varies with temperature, its value decreasing about 1 per cent. for a rise of temperature of  $4^\circ$  C.

The method of experiment was suggested by Professor Carey-Foster, in whose laboratory the experiments were made.

## LXI. *The Magnetic Circuit of Dynamo Machines.*

By W. E. AYRTON and JOHN PERRY.\*

[Plate IV.]

**I**N this paper we shall use the following symbols:—  
Dimensions are given in centimetres, current in amperes, potential differences and electromotive forces in volts, resistances in ohms

\* Communicated by the Physical Society: read March 10, 1888.



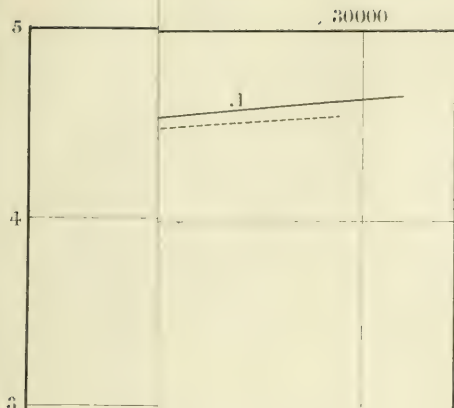
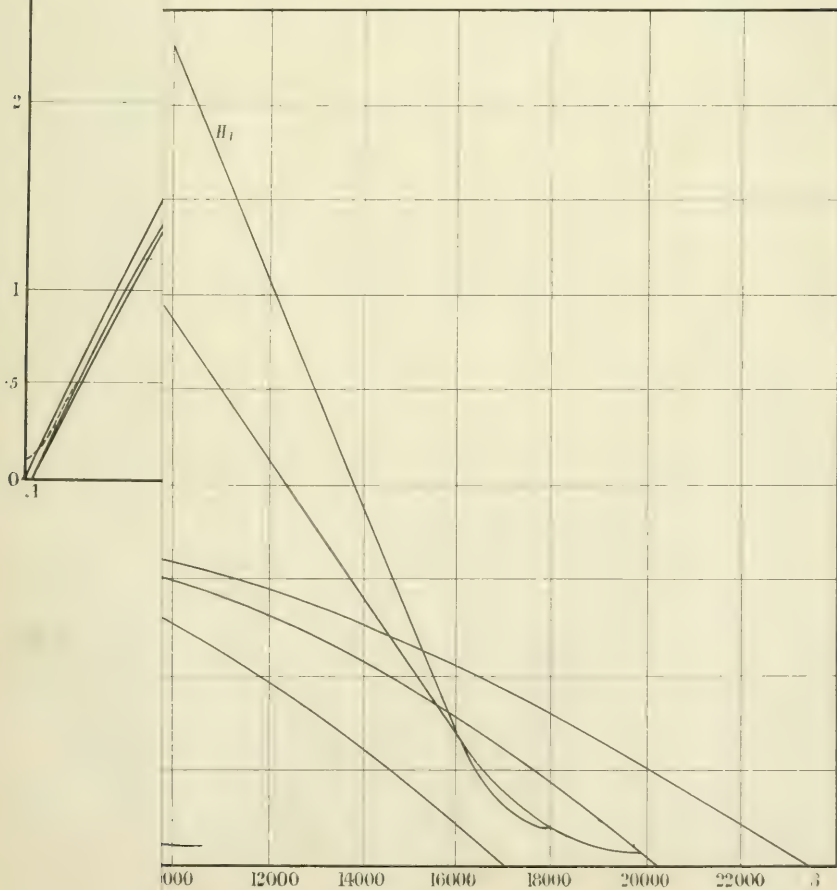
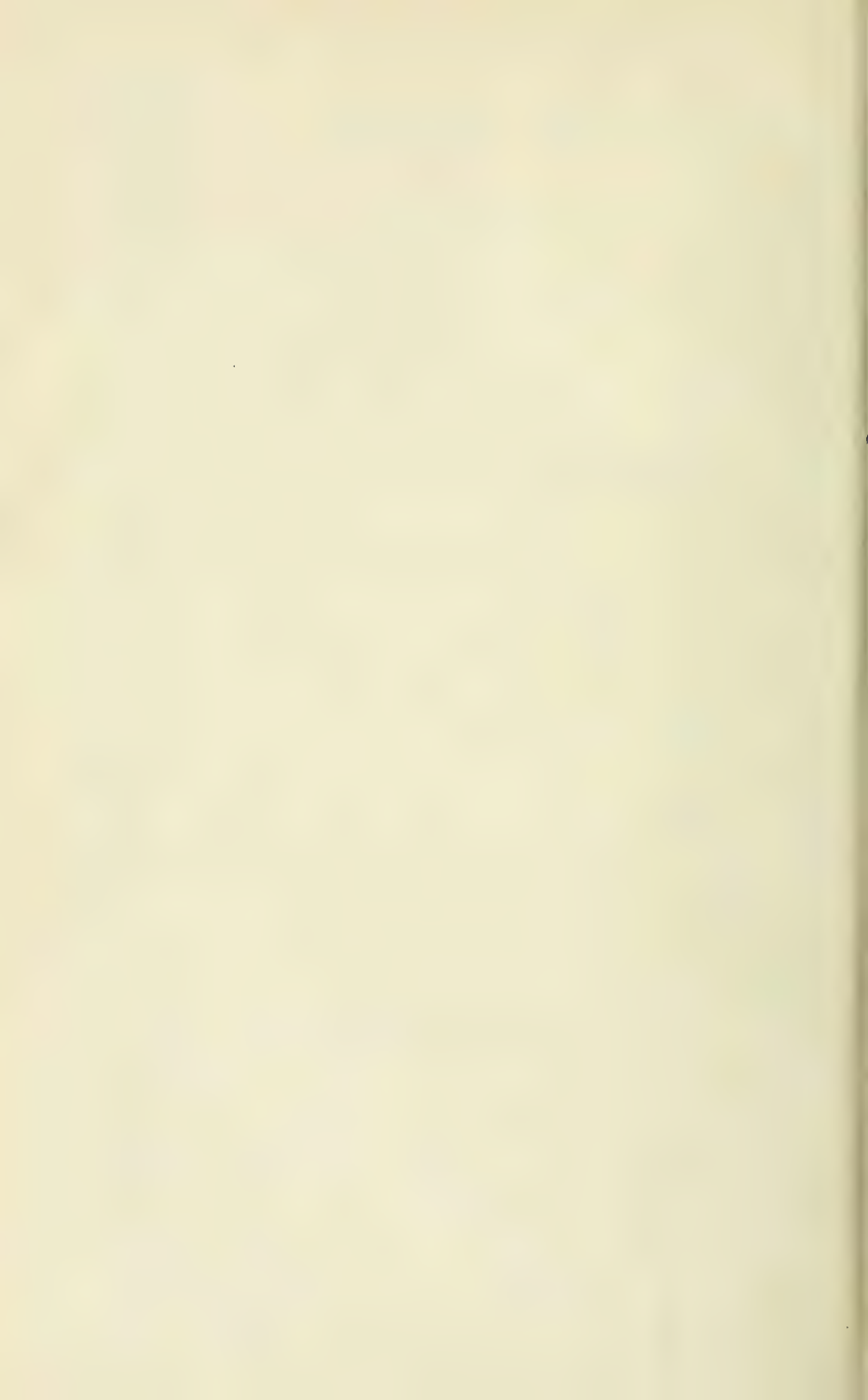


Fig. 1.





We shall speak indifferently of the drum or Hefner-Alteneck and the cylindric Gramme armature—that is, the Gramme armature which receives its induction through its cylindric surface from two pole-pieces. It is easy to make the slight changes in the formulæ required for flat gramme ring-armatures like those of the Victoria, Gulcher, and Schückert machines, which receive induction through their flat sides, and for all machines with more than two pole-pieces.

$r$ , outside radius of armature.

$L$ , length of armature parallel to the axis.

$k$ , a constant such that  $krL = a_1$ ;  $k$  is as much as 1.25 in drum or Hefner-Alteneck armatures, and as little as 0.5 in short cylindric or other Gramme armatures.

$t$ , the thickness of the winding of the armature.

$d$ , the clearance between wire and pole-piece.

$\delta = d + t$ , the total distance from iron of armature to pole-piece.

$n$ , the revolutions per second of the armature.

$v$ , the circumferential velocity of the armature in centimetres per second  $v = 2\pi rn$ .

$S$ , wires counting all round the outside of the armature.

$A$ , amperes in each wire.

$SA$  will be called the “ampere-wires” on the armature, being the ampere-turns if it is a Gramme, and being twice the ampere-turns if it is a Hefner-Alteneck.

$\alpha$ , amperes per sq. centim. flowing in the section of the armature-winding made by a plane at right angles to the axis.

$\alpha_1$ , the highest permanent value of  $\alpha$  allowable.

$\rho$ , the electric resistance of the armature-winding per cubic centimetre. That is, per centimetre of its length per sq. cm. of cross section. Of course  $\rho$  is greater than the specific resistance of copper, as the space is partly occupied by insulating material,  $\rho$  is greater with finer wire; but for some practical purposes it is advisable to assume  $\rho$  constant for all kinds of winding.

$\rho\alpha^2$ , watts developed as heat per cubic centimetre of the winding, so that total rate of development of heat  $= 2\pi r t L \rho \alpha^2$ .

$a_1$ , cross section of iron of armature by a diametral plane.

$a_2$ , area of pole-piece exposed to the armature, increased by a fringe of  $0.8 \delta$  in breadth all round.

$a$ , the cross section of any other portion of the magnetic circuit which may be considered.

$\beta$ , the induction in C.G.S. units, given in lines per square centimetre anywhere in the magnetic circuit.

$\beta_1$ , the value of  $\beta$  in the iron of the armature.

$\beta_1'$ , the greatest value of  $\beta$  which it is convenient to employ in the armature when the machine is giving its greatest permanent output.

$N$ , the total induction in the iron of the armature  $N = \beta_1 a_1$  or  $\beta_1 k r L$ .

$\frac{2\delta}{a_2}$  or  $\frac{2(t+d)}{a_2}$  will be called the magnetic air-resistance, a not very incorrect or misleading expression.

$\frac{l\nu}{a\mu}$  will be called the magnetic resistance of a portion of the iron part of the magnetic circuit whose length is  $l$ , whose cross section is  $a$ , the permeability being  $\mu$ ,  $N\nu$  being the total induction there.

The whole iron magnetic resistance may be written  $\sum \frac{l\nu}{a\mu}$ .

This is for only one of the magnetic circuits if the machine has more than one.

In a well-designed modern machine there is no throttling of the induction anywhere, and  $\frac{l}{a_1\mu}$  may be taken as

the whole magnetic iron resistance,  $\mu$  being the permeability of the iron of the armature,  $l$  the average total length of a line of induction in the iron of the whole machine.

$W$  watts, the power developed by the rotation of the armature.

$W'$ , the permanent highest power developed.

$E\theta$ , the rate of loss of heat in watts per square centim. of the cylindric outer surface of the armature. This is usually taken to be 0.2 in modern machines furnished with some ventilating arrangement. The value of

$\sqrt{\frac{E\theta}{\rho}}$  may, we find, be practically taken as 288 in

such machines; we shall call this constant  $q$ , as machines made by different makers differ greatly in its value, even where there are the same methods of ventilating and the same sizes of wires. With finer wires  $\rho$  is greater, as the space is less occupied by copper, so that  $q$  is less. If two thirds of the space be taken as occupied by copper,  $q = 288$  if  $E\theta = 0.2$ . In some armatures, which are always perhaps too safe from heating, we have found  $q$  to be as little as 150. In some cases it is considerably more than 300.



$S'$ , spires on the coils of the field-magnet part of one magnetic circuit. In the Edison-Hopkinson and Kapp machines there is only one magnetic circuit, and  $S'$  is here the total number of spires on the field-magnets; but in the Manchester, Crompton, A-Gramme, and other forms there are two magnetic circuits, and  $S'$  is here the number of spires on only one of the magnetic circuits.

$A'$ , amperes in each spire of the field magnet-coils; or

$\alpha'$ , amperes per square centimetre of cross section of the winding.

Any person who has engaged in making measuring-instruments or dynamo machines is aware that for a given volume of winding, whether the wires are small or large, for the same distribution of temperature there will be the same number of ampere-turns and there will be the same rate of loss of energy by heating, if the volume of insulating material is always in the same ratio to the volume of the copper. With fine wire the volume of insulating material becomes greater, but not to such an extent as to make useless this very important roughly correct practical rule for the makers of instruments and dynamo machines. This rule we have regularly used since 1881 in our measuring-instruments.

This has led us to speak of  $\alpha$  the current in amperes per square centimetre of cross section of a coil, rather than the density of current in the copper alone, and we have been led to some general rules of considerable practical interest in consequence.

To make our rules more complete, we begin with one which is well known. It will be observed that we use Mr. Kapp's method of counting wires on the armature, so as to make the rule suitable both for the Hefner-Alteneck and the Gramme armature.

$$\text{Total E.M.F. of armature} = \frac{nSN}{10^8} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\left. \begin{array}{l} \text{E.M.F. developed in unit length} \\ \text{of wire passing through the field} \end{array} \right\} = \frac{vk\beta_1}{\pi 10^8} \quad . \quad . \quad (2)$$

Observe that it is only the wire on the convex outer surface of the armature which is here considered.

$$W = \frac{2nN}{10^8} SA. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$W = \frac{2cNt\alpha}{10^8} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The heat generated in the armature-winding per cubic centim.  $= \rho \alpha^2$ . The possible ventilation arrangements are better in Gramme than in Hefner-Alteneck armatures, and it is usual, therefore, to consider only the heat generated in the wires on the outside convex surface of the iron of either. This is  $2\pi r t L \alpha_1^2 \rho$ ; and as the heat emitted is found, for the highest temperature  $\theta$  at which it is considered safe permanently to work the machine, to be proportional to the convex surface  $2\pi r L$ , if  $E$  is the rate of loss of heat in watts per square centimetre of this surface,

$$2\pi r t L \rho \alpha_1^2 = E \theta 2\pi r L,$$

or

$$t \rho \alpha_1^2 = E \theta.$$

We find that  $\frac{E \theta}{\rho}$  may be taken as 83000 in the best modern machines; we shall call this  $q^2$ , so that

$$t \alpha_1^2 = q^2. \quad . \quad . \quad . \quad . \quad . \quad (5).$$

We can, therefore, from (4), express the greatest permanent output of a machine  $W'$ , either in terms of  $\alpha_1$  or of  $t$ :

$$W' = 0.00166 \frac{v N'}{\alpha_1}, \quad . \quad . \quad . \quad . \quad . \quad (6)$$

$$W' = \frac{2q}{10^8} v N' \sqrt{t}, \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$W' = \frac{2q}{10^8} v k r L \beta_1' \sqrt{t}, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$N'$  and  $\beta_1'$  being the highest allowable values of  $N$  and  $\beta_1$ .

It is on the combination of (5) and (4) that we have based an important generalization regarding the magnetic circuit of the dynamo, which gives the title to our paper. Members of the Society are aware of the methods adopted by Mr. Bosanquet, Dr. Hopkinson, and Mr. Kapp in dealing with a magnetic circuit. Consider a closed tube of small cross section everywhere, passing through the iron of the armature, the air-spaces, the limbs of the field-magnet, and the yoke. Let the section vary so that the total induction is everywhere the same. Then, in a short length of the tube  $l$ , the line-integral of the magnetic force required to produce this induction is

$$\frac{l}{\mu} \beta,$$

where  $\mu$ , the magnetic permeability of the material at any place, is 1 for air, and where its value for iron is given in

the accompanying figure for various values of  $\beta$ . The curve B, fig. 1, is that given by Mr. Bidwell in his paper (Proc. Royal Society, No. 245, 1886). The curve  $H_1$  has been obtained from Dr. Hopkinson's experiments on wrought iron in the Philosophical Transactions, 1885, plate 47. We wish that Dr. Hopkinson had in this paper given his experimental numbers, instead of plotting them in curves to such scales that one feels very great uneasiness in making measurements. We have determined the curve  $H_1$  by taking a medium curve between Dr. Hopkinson's curves for rising and falling magnetizations. It is quite probable that, in view of the great differences in the behaviour of the same and different kinds of iron, under different circumstances, Dr. Hopkinson did not feel that he was justified in drawing his curves on paper too finely divided. It will be observed that from  $\beta=10000$  to  $\beta=16300$  we may take  $\mu=5221-0.3071\beta$ . Unfortunately for easy methods of calculation in many machines much larger values of  $\beta$  than 16300 are often used.

Curve  $H_2$  is computed from Dr. Hopkinson's results for cast iron, and it shows how very important it is, when cast iron is used in the magnetic circuit of a machine, to have its section much greater than that of the wrought iron portions of the circuit, if throttling the induction is to be avoided.

Curves  $K_1$ ,  $K_2$ , and  $K_3$  are computed from the formulæ given in Mr. Kapp's paper for the wrought iron used in the armature, field-magnets, and yoke respectively of a dynamo machine.

The differences among these curves throw great light upon the fact that it is really impossible to predetermine the "characteristic" of a dynamo machine. Mr. Kapp gets over discrepancies by calculating a value of his leakage-resistance which suits the actual observations made on an already constructed dynamo machine; and Dr. Hopkinson's curves, although calculated on the basis of actual measurements of the leakage made on an already constructed dynamo, and although only computed for a very small portion of that part of the characteristic where the iron magnetic resistance is important (the only portion about which there is any difficulty), represent this small portion very indifferently indeed. Methods of calculating the leakage which have been put forward we can in no way believe in, for reasons given by one of us in the discussion on Mr. Kapp's paper at the Society of Telegraph Engineers. We are not in any way detracting from the merits of Dr. Hopkinson's beautiful theory. We are too well aware of the great services he has done us and of the

enormous change which he has produced in the construction of dynamos.

Now the line-integral of the magnetic force round a closed magnetic circuit—called by Mr. Bosanquet the Magneto-motive Force—is the number of ampere-turns on the coils through which it threads its way  $\times \frac{4\pi}{10}$ . Hence if  $S_2A_2$  be the ampere-turns on the field magnet,

$$\frac{4\pi}{10} S_2A_2 = 2\delta\beta + \sum \frac{l}{\mu} \beta, \dots \dots \dots (9)$$

$\delta$  being  $d+t$  the distance from iron of armature to iron of field-magnet, and of course the length of the tube in air is twice this distance as it goes into and out of the armature.

If  $N$  is the total induction through the iron of the armature,  $\frac{N}{a_1} = \beta$  there and  $\frac{N}{a_2} = \beta$  in the air-space.

If  $\nu N$  is the total induction through the iron of the field-magnet anywhere, then  $\frac{\nu N}{a} = \beta$  there, so that (9) may be written

$$\frac{4\pi}{10} S_2A_2 = 2(d+t) \frac{N}{a_2} + \sum \frac{l\nu N}{a\mu},$$

$$\text{or} \quad N = \frac{\frac{4\pi}{10} S_2A_2}{\frac{2(d+t)}{a_2} + \sum \frac{l\nu}{a\mu}}; \dots \dots \dots (10)$$

and as  $N$  is the induction through the armature produced by a magneto-motive force  $\frac{4\pi}{10} S_2A_2$ , equation 10 leads to  $\frac{2(d+t)}{a_2}$  being called, by analogy with other physical resistances, the air magnetic resistance, and  $\sum \frac{l\nu}{a\mu}$  the iron magnetic resistance of the circuit. The members of the Society will perhaps allow us to put in this way Dr. Hopkinson's theory, although he himself may object to some of the terms we use.

Now let it be assumed that there is a value of  $\beta$  for the armature iron which it is best to use in all machines when giving their permanent output.  $\nu$  may also be taken as practically constant. We are aware that to both these assumptions exception may be taken, but this will not be found to affect the practical general result which we arrive at.



Then  $\mu$  is known, so that if the lengths and cross sections of the iron everywhere are known, the iron magnetic resistance  $\sum \frac{l\nu}{a\mu}$  is known. Inserting this value of  $N$  as  $N'$  in (7) we find

$$W' = \frac{2q}{10^8} v \sqrt{t} \frac{\frac{4\pi}{10} S_2 A_2}{\frac{2(d+t)}{a_2} + \sum \frac{l\nu}{a\mu}}; \quad \dots \quad (11)$$

and it will be found that this is a maximum for different values of  $t$  when

$$\frac{2t}{a_2} = \frac{2d}{a_2} + \sum \frac{l\nu}{a\mu} \dots \dots \dots (12)$$

That is, when the magnetic resistance of the space occupied by the winding of the armature is equal to the resistance of the rest of the magnetic circuit.

When this is the case (11) reduces to

$$W' = \frac{2\pi q}{10^8} \frac{v a_2}{\sqrt{t}} S_2 A_2. \quad \dots \dots \dots (13)$$

That the ampere-turns on the field-magnet  $S_2 A_2$  may really produce the given induction  $\beta_1$  in the armature, it is necessary that

$$S_2 A_2 = \frac{10}{\pi} \frac{a_1 t}{a_2} \beta_1;$$

and if we insert this value in (13) we have of course (7) again.

It is now the custom in making dynamos to let  $\beta$  be nearly constant in all parts of the circuit where the iron is the same, and to have  $\frac{\beta}{\mu}$  as nearly constant as possible, if different kinds of iron are employed. If  $\frac{\beta}{\mu}$  is larger anywhere than its average value for the rest of the circuit, we say that the induction is there "throttled," and throttling is only allowable in the armature, if it is allowable anywhere. If  $\frac{\beta}{\mu}$  is nearly constant everywhere in the iron, we may take  $\frac{l}{a_1 \mu}$  as the total iron resistance of the circuit,  $l$  being the average length of the lines of magnetic induction in the iron of the whole circuit, and then we may use

$$\frac{l}{a_1 \mu} \text{ instead of } \sum \frac{l\nu}{a\mu}$$

in (10), (11), and (12);  $\mu$  being the permeability of the armature iron when the induction is  $\beta_1'$ .

In the above investigation, the iron resistance is supposed to be given, and also the exciting power in ampere-turns. At present this seems a sufficiently practical basis for the calculation, as we usually fix first the size of armature, then arrange as short a magnetic circuit as possible, which must leave sufficient room for the exciting coils. We see, however, that a larger generalization is possible when we know with certainty what is the limiting thickness of winding on the field-magnets. We were allowed to assume in the armature winding that  $t\alpha_1^2$  is constant,  $\alpha_1$  being the greatest permanent current per square centimetre allowable. When the winding is thin, so that the temperature is nearly uniform in the winding, and only when this is the case, is such a rule allowable, and it is not allowable in cases where the winding is of the thickness usual in field-magnet coils. We are at present experimenting on this subject, but there are considerable difficulties in the way of obtaining practical rules. We have no doubt, however, that there is such a rule as

$$S_2'A_2' = p\lambda.$$

That is, the greatest number of ampere-turns which can usefully and permanently be applied on a field-magnet coil of length  $\lambda$  is proportional to  $\lambda$ . If  $l$  is given, the configuration of the machine enables  $\lambda$  and therefore  $S_2'A_2'$  to be fixed.

Then  $S_2'A_2' \div \frac{2l}{\mu_1} = \beta_1$  enables  $\beta_1$  and  $\mu_1$  to be calculated by trial if there is sufficient information about the character of the

iron.  $\frac{2t}{a_2}$  is now made equal to  $\frac{2d}{a_2} + \frac{l}{a_1\mu_1}$ , so that the important dimensions are fixed. Should  $\beta_1$  be great, considerations of cost of the field-magnet winding may come in, to alter completely the design of the machine, but for a given configuration of machine this is the practical method of working.

From experiments, the results of which were published before this Society on March 12th, 1887, we came to the conclusion that there is a definite resistance at a joint in a magnetic circuit; and Professor Ewing found that by cutting a bar into two, four, and eight pieces, the magnetic permeability seemed to alter from 1220 to 980, 640, and 480 respectively, the joints being tooled up in the usual way. When the joints are carefully scraped he also found that they materially increased the magnetic resistance unless considerable pressure was applied. Now as a stress of less than one quarter of the ordinary stresses to which wrought iron is

subjected in machinery materially diminishes the permeability of iron, it is obvious that all joints in the magnetic circuit ought, as much as possible, to be got rid of, consistently with easy manufacture; or, when they exist, they ought to be made of as large area as possible, by letting one piece into another to a considerable depth.

In the design of a machine, the question of possible leakage is of considerable importance. It will often be found that in the effort to make  $l$  short and  $a_2$  large, machines are designed with enormous amounts of leakage not only from one pole to the other, but from either pole to the middle of a neighbouring field-magnet coil.

One of the best ways of finding the nature of the probable magnetic leakage in a dynamo, before it is constructed, is to construct a small model of the same kind of iron, exciting the field-magnets, and exploring by means of a ballistic galvanometer. Another simpler way, which gives a considerable amount of information, and which we have employed, is to make a model of wood, covering certain judiciously selected parts, such as the poles and armature, and half the field-magnets with metal, immerse the model in a barrel of rain-water, and find the electric resistance between one part and another when electric potential-differences are established between them. On account of the ease with which the model may be rearranged in configuration, this method of working gives interesting results; but these results, when applied in the magnetic case, must be used judiciously and with the knowledge that the permeability of iron is not a fixed quantity.

It is obvious that the best section of a field-magnet limb is the circular, but considerations of possible leakage to the middle of the limb from the armature or a pole-piece, and other considerations relating to the configuration of the machine, often cause us to give to the section a rectangular or oval section.

### *The Characteristic of a Dynamo.*

In a letter written by one of us from Japan, in January 1879\*, before he had seen a dynamo, the necessity was shown for establishing an algebraic relation between the E.M.F. developed in the armature and the current exciting the field-magnets. It was pointed out that  $E$  the E.M.F. in the armature was proportional to the field and speed, and that this led to

$$E = p + qc + \Sigma \frac{ac}{b+c} \dots \dots \dots (14)$$

\* Not published till 1885, and then in a somewhat mutilated form in the 'Electrician' of November 20.

for any given speed. It was also pointed out that a tangent function might also be employed, but that it would not lend itself so readily to calculation. We had, both, in 1878 used a curve to express this relationship, but it was not until 1881, when we met M. Deprez, and learnt of his work, that we had any conception of the many calculations which might be made by graphical methods, using the curve as a fundamental relation. Herr Fröhlich uses one term of the above expression:—

$$E = \frac{ac}{b+c} \cdot \dots \dots \dots (15)$$

He regards  $p$  the permanent field as 0, and he has no term which remains proportional to the current. We shall speak of (15) as the Fröhlich formula, because it usually goes under that name. It is, of course, an empirical formula, like (14).

The reasoning which led us to regard (14) as a *rational* formula, was based on a magnetic theory which need not here be expounded.

A very great necessity exists for having some such empirical formula as (15), but, as is well known, it is quite incorrect when  $c$  is small. It is not nearly so incorrect as we might imagine it to be from the statements made by Mr. Kapp (p. 529, Journ. Soc. Tel. Eng. and Elect. vol. xv. 1887), who has given in his fig. 3 a most absurdly unsuitable Fröhlich curve, or by Dr. Hopkinson (plate xvi. Phil. Trans. 1886), whose Fröhlich is also not the most suitable. It is obvious that the Fröhlich ought not to be expected to agree with the real characteristic near the origin. It ought to be made to agree most perfectly with the working part of the characteristic. To effect this purpose let the observed values

of  $\frac{E}{C}$  and  $E$  be plotted as the coordinates of points on squared paper. The straight line which lies most evenly among the points for the working values of  $E$  satisfies the equation

$$\frac{E}{C}b + E = a.$$

The measured coordinates of two points on this straight line enable  $b$  and  $a$  to be calculated.

Thus, for example,

$$N = \frac{1364 F}{1 + 2.7 \times 10^{-4} F}$$

will be found to satisfy the observations published by the Drs. Hopkinson for the Manchester dynamo from  $F=6000$  to  $F=30,000$  with a wonderful amount of accuracy. But for



the early part of the curve it is quite unsuitable.  $F$  is  $\frac{4\pi}{10} \times$  ampere-turns in one coil of the machine, or the magneto-motive force.

The uses made by Herr Fröhlich and Prof. Rücker of (15) in a theory of compounding show how important it is to have a simple empirical formula. Indeed in our patent specification of 1882 we base the theory of compounding on the simplest of all empirical formulæ,

$$E = aC. \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

And it is by means of this formula that the theory can be best put before students ; if our easy reasoning is properly grasped, the practical electrical engineer will be able to use the results graphically with actual characteristics in the manner employed by the most experienced men at the present day.

Instead of speaking of  $E$  the E.M.F. produced in the armature, which is proportional to  $N$  the magnetic field and  $n$  the speed, as shown in (1) it is preferable to speak of  $N$  the field itself. And instead of referring merely to the current exciting the field-magnets, it is preferable to speak of the ampere-turns  $S_2A_2$ , or, better, of the magneto-motive force in C.G.S. units,  $\frac{4\pi}{10} S_2A_2$ .

We have tried in vain, during perhaps two months of very hard work, to express the two constants  $a$  and  $b$  of (15) in terms of the dimensions of a dynamo machine. We now believe that this cannot be effected. We have already given the reason why the linear law connecting the  $\beta$  and  $\mu$  cannot be used in obtaining a formula. In any case such a formula would be very different from that of Fröhlich. But in view of the important relation (12) which we have established, that when the machine is giving its best permanent output,—

The air magnetic resistance of the space occupied by winding on the armature = all other magnetic resistance,

it is not difficult to arrive at practical rules of working. It will be noticed that in existing machines we may, in general calculations, neglect the resistance of the clearance-space  $d$ .

The most important fact to be kept in mind by constructors of dynamos is, that the magnetic air-resistance is the governing factor. Until half the highest induction is reached we may neglect altogether the resistance of the iron ; and indeed for many calculations it is sufficient to take

$$N = \frac{4\pi}{10} \frac{a_2}{28} S_2A_2 ; \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

where  $a_2$  is the area of the pole-piece exposed to the armature (increased by the fringe-area, which Dr. Hopkinson estimates to be of a breadth  $0.8\delta$ ), and  $\delta$  is the distance from iron of armature to iron of pole-piece.

(17) gives, in fact, the straight part of the characteristic. In well-constructed dynamos the magnetic iron resistance is equal to the air resistance  $\frac{2\delta}{a_2}$  when the machine is working at its most permanent output; and it is only necessary to know  $\beta'$ , the induction convenient to use when this is the case, to be able to calculate  $N$  and  $S_2A_2$  for a point on the curved part of the characteristic. Thus

$$a_1\beta_1 = N_1 = \frac{4\pi}{10} \frac{a_2}{4\delta} S_2A_2;$$

so that if  $a_1$  and  $\beta_1$  are known,  $S_2A_2$  can be found. Thus in the Manchester dynamo described by Dr. Hopkinson (Trans. Roy. Soc. 1886),

$$a_1 = 220.5 \text{ square centim.,}$$

$$a_2 = 839.5 \quad , , \quad , ,$$

$$\delta = 0.8 \text{ centim.}$$

Taking  $\beta_1 = 18,460$  lines per square centim., we find

$$a_1\beta_1 = 4.07 \times 10^6.$$

Now

$$\frac{4\pi}{10} \frac{a_2}{2\delta} = 660.$$

So that  $N = 660 S_2A_2$  represents the straight part of the characteristic.

Again,  $N_1 = a_1\beta_1 = 4.07 \times 10^6$ ,  
and

$$a_1\beta_1 = 330 S_2A_2,$$

or

$$S_2A_2 = \frac{4.07 \times 10^6}{330} = 12330.$$

Plotting  $N = 4.07 \times 10^6$  and  $S_2A_2 = 12330$  as the coordinates of a point on squared paper, and plotting also the straight line from the origin  $N = 660 S_2A_2$ , a man who has seen characteristics before, will be able to draw the characteristic of this machine with a fair amount of accuracy, especially if he recollects that the iron is near saturation. Those, however, who have less experience may find the Fröhlich, which for any dynamo passes through the point on the straight line which represents  $N = 10,000 a_1$ , and the second point just found,

rounding off the straight line into the Fröhlich by hand. In applying our rule to the Manchester dynamo we employed neither of these methods, as one's judgment is vitiated when one has seen the actual observations; and we have assumed that the very roughly correct rule,

$$N = p \tan^{-1} (h \cdot S_2 A_2),$$

may be applied, making its slope the same as that of the straight line at the origin, and making it pass through the point whose position we have calculated. In this case

$$ph = 660;$$

and it will be found that

$$p = 34.65 \times 10^5,$$

and

$$h = 19.03 \times 10^{-5}$$

for this case.

As Dr. Hopkinson has given his characteristics, not in terms of ampere-turns  $S_2 A_2$ , but in absolute units, calling  $\frac{4\pi}{10} S_2 A_2$  by the letter  $F$  to denote the magneto-motive force in absolute units, the above formula becomes for this comparison,

$$N = 34.65 \times 10^5 \tan^{-1} (15.15 \times 10^{-5} F).$$

In the figure we have plotted the curve obtained by calculation as A; B shows Dr. Hopkinson's calculated curves; the dotted line passes as nearly as possible through the points determined by experiment. It will be observed that the Drs. Hopkinson have only ventured to calculate the characteristic up to  $F=12,000$  on the ascending, and 9500 on the descending part; whereas the observations extend to  $F=29,500$ . As the very straight part near the origin presents no difficulty, it may be said that all their elaborate calculation represents only the characteristic from about  $F=6000$  to  $F=12,000$ , and that part very indifferently. It is, besides, to be observed that in an actual dynamo there are no such differences between observations made with steadily increasing and with steadily diminishing magneto-motive forces, as the theory requires.

In the above calculation we have neglected the clearance  $d$ , as we did not know it. It would have been easy to calculate a value of  $d$ , possibly not very different from the real value, to make our curve agree more closely with the observations. In conclusion we would say that, in our opinion, it is impossible, until a machine is constructed, to compute its characteristic

with sufficient accuracy for such purposes as the determination of coils in compound winding. But the probable characteristic may be determined in the way here described, with sufficient accuracy for a number of useful purposes. It is based on the facts:—(1) When a machine is working at its best permanent output, its iron magnetic resistance plus the air magnetic resistance of the clearance is equal to the air magnetic resistance of the space on the outside of the armature occupied by winding. (2) At the beginning the air-resistance is alone of any importance.

LXII. *On the Recalescence of Steel.* By H. F. NEWALL, M.A., *Demonstrator in Experimental Physics, Cambridge University*.\*.

IN a Note published in the Philosophical Magazine for November 1887, I stated the results of a number of experiments made with a view to investigate the recalescence of steel. I propose in the present note to give an account of the experiments on which I base those results.

I. Reglow, or recalescence, is not due to chemical action at the surface of the steel, nor is it due to occlusion of gases.

A steel wire with copper terminals was put in a glass tube, the terminals passing through indiarubber stoppers, which closed the tube tightly at its two ends. The tube was exhausted by means of a mercury-pump till the air-pressure was a very small fraction of a millimetre. The steel wire was heated again and again by the passage of an electric current from storage-cells. Pressure rose slightly after the first three or four heatings (probably because of the setting free at high temperatures of the air-layers condensed on wire and tube surfaces), and was reduced by further pumping. Both darkening and reglow were observed with undiminished distinctness after fourteen heatings, and the process was not repeated. Nitrogen was admitted to the tube and produced no effect that could be detected on the brilliancy of reglow.

II. Reglow is not due to difference in conductivity at different temperatures, as suggested by Forbes.

A wire (.5 millim. diameter) was hammered flat and still showed reglow. A thin steel plate, hammered till its thickness was less than .1 millim., as measured by a screw-gauge, still showed reglow. Either we must believe that there can

\* Abstracted from a paper read before the Cambridge Philosophical Society, January 29, 1888. Communicated by the Author.



be the considerable difference of temperature evidenced by reglow, within .05 millim., or we must relinquish the ingenious theory suggested by Prof. Forbes. See also next paragraph.

### III. A rise of temperature takes place throughout the mass.

A rod of steel, 1 centim. in diameter and 7 centim. long, was softened, and a hole, 1 millim. in diameter and 3 centim. long, was drilled down its axis. A thermoelectric couple of PtCu, composed of wires No. 36 B.W.G., was inserted, precautions being taken that there should be contact with the tube only at the junction of Pt and Cu, if at all. The free ends of the couple were attached to a galvanometer which was most satisfactorily dead-beat, a vane dipping in water being attached to the magnet and mirror. The galvanometer-indications were those denoting unmistakable rise of temperature with the reglow during the cooling, and a fall of temperature with the appearance of "darkening" in the heating.

Considerable difficulty was experienced in showing the actual change of *sign* in the temperature-variations, until it was realized that care must be taken to apportion the proper masses to the PtCu wires and the steel tube. If the wires were larger than No. 30 B.W.G., only a *pause* in the rise or fall of temperature was observed; the thermoelectric couple did not truly denote the temperature of the enclosure, the heat coming into the couple being conducted along the thick wires too quickly to admit of a rise of temperature at the actual junction. This was obviated by enclosing the steel tube in a massive brass tube in order to diminish the rate of cooling of the steel, and also by making the thermoelectric couple with finer wire; the latter plan being found most satisfactory, inasmuch as the reglow could be easily observed. The galvanometer-indications denote that the temperature of darkening is higher than that of the reglow.

### IV. Peculiarities in thermoelectric properties.

Allowances being made for differences in magnitude, the peculiarities exhibited by a thermoelectric couple of steel and platinum are similar to those exhibited by the PtCu couple when heated or cooled in a steel tube, as just described. In fact, part certainly of the peculiarities observed and graphically represented by Prof. Tait may be explained by the rise of temperature in the cooling steel, which may, I think, now be taken as proved.

The same difficulties that I have experienced in proving the rise of temperature inside a block of steel, as a certain point is reached in the cooling of the block, would be met with in

an exaggerated degree in measuring the temperature of a couple of which one element is steel, by means of a second couple placed inside an enclosure side by side with the steel couple. The accession of heat in the enclosure due to the reglow of the steel in the thermoelectric couple is not enough to produce an effect on the thermometric couple.

There is another peculiarity in the thermoelectric properties of steel, to which Mr. Tomlinson refers (*Phil. Mag.* Jan. 1888). A piece of steel wire is connected up with a galvanometer, and is heated by a flame at a point remote from the galvanometer connexions. There are no signs of a resultant E.M.F. in the circuit, even when the flame is moved, until 'darkening' has occurred. Then, however, if the flame be moved along the wire so that reglow takes place behind it in the cooling part, and darkening in front in the part being heated, an E.M.F. results, and persists only so long as the flame is kept moving.

In proving the rise of temperature throughout the mass of steel during recalescence, I have shown that the temperature of recalescence is different from that of darkening, the latter being higher. This difference of temperature of what may be regarded as the junctions of the hot altered steel with the cold steel on each side, determines the E.M.F. in the circuit.

LXIII. *On an Extension of Carnot's Theorem.* By J. PARKER, B.A., late Scholar of St. John's College, Cambridge\*.

IT is well known that no engine can be more efficient in transforming heat into mechanical work than Carnot's reversible engine, and that all reversible engines are of the same efficiency. It is also generally believed, without, however, being proved, that no irreversible engine can be so efficient as Carnot's. This is the point we propose to examine.

One of the principal conditions imposed on a system subjected to a reversible operation is that, at every instant, the system is in mechanical and thermal equilibrium. This condition, however, also belongs to a certain class of irreversible processes, as we shall presently see.

Thus, when a saturated solution of sodium chloride is heated with an excess of the salt, the salt is dissolved; but if the temperature is again reduced, the solution deposits, not the anhydrous salt, but crystals of the bihydrate. Again, a salt is neutral to pure ice at the freezing temperature of its cryohydrate; and if heat is slowly imparted, the liquid cryohydrate

\* Communicated by the Author.

will be formed at this constant temperature. The system is in mechanical and thermal equilibrium throughout the process; but the process is not reversible, for if heat is slowly abstracted from the liquid cryohydrate, it is not dissociated—it is simply frozen.

We define an “equilibrium path” to be a path at every point of which the system under consideration is in equilibrium, which may be either stable or unstable, provided, of course, that the equilibrium is not broken at any point of the path.

A reversible path is always an “equilibrium path,” but an “equilibrium path” is not necessarily reversible, as we have already seen. All the irreversible equilibrium paths I have noticed correspond to an absorption of heat—that is, an increase of entropy.

Now it is well known that in a complete cycle the energy dissipated  $= -t_0 \int \frac{dq}{t}$ , where  $dq$  denotes a quantity of heat absorbed, and  $t_0$  is the temperature of the refrigerator. Clausius has shown that this quantity cannot be negative, and that when the cycle is reversible it is equal to zero. It is then generally assumed that in all irreversible cycles the energy dissipated has a positive value.

The study of Solubilities has compelled me to adopt a different view. I have been led to conclude that in any irreversible equilibrium-cycle the energy dissipated is zero, but that in all other irreversible cycles the energy dissipated has a positive value. Any irreversible equilibrium-engine is, then, as efficient as Carnot's perfectly reversible engine, and all other irreversible engines are less efficient than Carnot's.

We have already seen that  $\int \frac{dq}{t} = 0$  for any equilibrium-cycle. Suppose, then, that a system travels from one position A to another position B by an irreversible equilibrium path, and let the cycle be completed by the reversible path BCA. It has been shown by Clausius that, for the reversible path BCA,

$$S_A - S_B - \int_B^A \frac{dq}{t} = 0;$$

where  $S_A$  is the entropy of the system in the position A, and  $S_B$  the entropy in the position B. But  $\int \frac{dq}{t} = 0$  for the complete cycle: hence, for the irreversible equilibrium path AB,

we have

$$S_B - S_A - \int_A^B \frac{dq}{t} = 0.$$

Thus, if the system can pass from the position A to the position B by several irreversible equilibrium paths, then  $\int_A^B \frac{dq}{t}$  has the same value for all the paths.

Again, when the temperature is constant throughout the modification, the heat absorbed will be

$$t(S_B - S_A).$$

An example of this will be found in equation (4), page 408 (May 1888).

In particular, consider the formation of unit weight of a liquid cryohydrate under a pressure equal to the pressure of the saturated vapour of ice. The cycle may be completed by the following operations, performed at constant temperature and pressure :—

(1) Let the cryohydrate be dissociated by evaporation into the saturated vapour of ice and the anhydrous salt or a hydrate. In the latter case, the cryohydrate is supposed to have been formed from the hydrate and ice.

(2) Let the aqueous vapour be separated from the salt, and then condensed into the solid state.

Since the pressure is constant throughout the cycle, the total work done and heat absorbed are both zero.

But if S be the entropy of the cryohydrate, and  $S_1$  the sum of the entropies of the salt and ice of which it is composed, the heat absorbed in the reversible parts of the cycle will be

$$t \cdot (S_1 - S).$$

Hence the heat absorbed in the formation of the cryohydrate—an irreversible process—will be

$$Q = t \cdot (S - S_1).$$

#### LXIV. *Notices respecting New Books.*

*Transactions of the Edinburgh Geological Society*, Vol. V., Part III. (Pp. 335-483.) Edinburgh, 1887.

AN interesting inaugural address by Mr. Ralph Richardson, treating of the discoveries of Fossil Mammalia in Scotland, with a map and lists of the places at which the individual species have been found, commences this number of the 'Transactions.' Following this, Mr. John Henderson treats of the Sands and Gravels of Musselburgh and Stockbridge, containing remains of



trees; and in a second paper the same author describes an ancient Lake-basin at Holyrood. Other papers on Stratigraphical Geology are given:—Mr. Hamilton Bell writes “On Exposures of the Old Red between Callender and Crieff;” Mr. T. Wallace, “Upper Stratherrick;” and Mr. M'Diarmid, “On a Boring at Dumfries.” In this boring, at 149 feet from the surface, a bed of Permian breccia, composed of fragments of Silurian Rocks, Porphyry, and Felstone, was met with, and proved to be 283 feet in thickness.

The Palæontological papers include:—Mr. Tait Kinnear, “On the genus *Fenestella*,” and “On a new Crustacean from Ardross.” The first paper deals with the revision of species of this typically Carboniferous genus of Polyzoa; and the second notices a new fossil Schizopod for which Mr. Peach proposes the name *Rostrocaris*. Mr. James Thomson contributes one of his illustrated papers on the Corals—with figures of the genus *Lithrostromion*; and Mr. John Young discusses Ulrich's *Cystodictyonidæ*, a family of Carboniferous Polyzoa.

A Petrological paper is given by Mr. A. Johnstone, who treats of the “Evolution and Classification of Igneous Rocks;” while Physical Geology includes a paper by Dr. Black on “Brighton Beaches after Storms of October and December 1886,” and an address by Mr. James Melvin on “Hutton's views of the Vegetable Soil or Mould, and Vegetable and Animal Life.”

Perhaps, however, the most important contribution to the present number is Prof. E. A. Claypole's paper on “The Lake Age in Ohio.” By a careful study of the glacial deposits in the Northern United States, Prof. Claypole is enabled to map out the old Lake-Basins caused by the Ice-dams and other obstructions, his views being rendered much clearer by a series of sketch-maps which accompany the paper. These old lakes were apparently much larger than any now existing, their size and extent varying constantly, according to the advance, retreat, or breaching of the Ice-dams. The nature and violence of the glacial floods are fully discussed, and attention is drawn to the enormous and often repeated denudation which occurred during this period.

## LXV. *Proceedings of Learned Societies.*

### GEOLOGICAL SOCIETY.

[Continued from p. 447.]

April 25, 1888.—W. T. Blanford, LL.D., F.R.S., President,  
in the Chair.

**T**HE following communications were read:—

1. “Report on the Recent work of the Geological Survey in the North-west Highlands of Scotland, based on the field-notes and maps of Messrs. Peach, Horne, Gunn, Clough, Hinxman, and Cadell.”

At the outset a review was given of the researches of other

observers, in so far as they forestalled the conclusions to which the Geological Survey had been led. Reference was made to the observations of Macculloch, Hay Cunningham, C. W. Peach, and Salter; to the prolonged controversy between Sir Roderick Murchison and Professor Nicol; to the contributions of Hicks, Bonney, Hudleston, Callaway, Lapworth, Teall, and others. It was shown that Nicol was undoubtedly right in maintaining that there was no conformable sequence from the fossiliferous quartzites and limestones into the eastern schists. It was also pointed out that the conclusions of Professor Lapworth regarding the nature and origin of the eastern schists involve an important departure from Nicol's position, and are practically identical with those obtained independently by the Geological Survey.

The results of the recent survey work among the Archæan rocks may be thus summarized:—(1) the eruption of a series of igneous rocks of a basic type in which pegmatites were formed; (2) the development of rude foliation in these masses, probably by mechanical movement, and their arrangement in gentle anticlines and synclines, the axes of which generally run N.E. and S.W.; (3) the injection of igneous materials, mainly in the form of dykes, into the original gneisses, composed of (*a*) basalt rocks, (*b*) peridotites and palæopierites, (*c*) microcline-mica rocks, (*d*) granites; (4) the occurrence of mechanical movements giving rise to disruption-lines trending N.W. and S.E., E. and W., N.E. and S.W.; (5) the effects of these movements on the dykes were to change the basalt-rocks into diorites and hornblende-schists, the peridotites and palæopierites into talcose schists, the microcline-mica rocks into mica-schists, and the granites into granitoid gneiss; (6) the effects on the gneiss resulted in the formation of sharp folds trending generally N.W. and S.E., the partial or complete reconstruction of the original gneiss along the old foliation-planes, and finally the development of newer schistosity more or less parallel with the prominent disruption-lines.

There is an overwhelming amount of evidence to prove that all these various changes had been superinduced in the Archæan rocks in Pre-Cambrian time.

After reviewing the facts bearing on the denudation of the Archæan land-surface, the order of succession and thickness of the Cambrian strata were given, from which it is apparent that the deposits gradually increase in thickness as we pass southwards from Durness to Loch Broom.

Prior to the deposition of the Silurian sediments the Cambrian strata were folded and extensively denuded. By these means various Cambrian outliers were formed far to the east of the present limits of the formation.

The order of succession of the Silurian strata along the line of complicated structure from Eriboll to Ullapool was described, reference being made to the further subdivision of the "Pipe-rock" and the Ghrudaidh Limestones (Group I. of Durness section). None of the richly fossiliferous zones of Durness is met with along this line, as they occupy higher horizons. An examination of the fossils

recently obtained by the Geological Survey from the Durness Limestones confirms Salter's conclusions that they are distinctly of an American type, the Sutherland quartzites and limestones being represented by the Potsdam Sandstones and Calciferous Sand Group of North America.

After the deposition of the limestones, the Cambrian and Silurian strata were pierced by igneous rocks, mainly in the form of sheets, producing important alterations in the sedimentary deposits by contact-metamorphism, the quartzites becoming crystalline, and the limestones being converted into marble.

When this outburst of volcanic activity had ceased, terrestrial displacements ensued on a stupendous scale. By means of powerful thrusts the Silurian strata were piled on each other, and huge slices of the old Archæan platform, with the Cambrian and Silurian strata resting on it, were driven westwards for miles. With the view of illustrating the extraordinary complications produced by these movements, a series of horizontal sections was described drawn across the line between Eriboll and Ullapool.

The evidence relating to regional metamorphism was next referred to, from which it is obvious that with each successive maximum thrust there is a progressive amount of alteration in the displaced masses, as the observer passes eastwards to the higher thrust-planes. Eventually the Archæan gneiss is so deformed that the Pre-Cambrian foliation disappears and is replaced by new divisional planes; the Cambrian grits and shales are converted into schists; the Silurian quartzites into quartz-schists; the limestones become crystalline; the sheets of intrusive felsite, diorite, and granitoid rock pass into sericite schist, hornblende-schist, and augen-gneiss respectively.

These researches furnish a vast amount of evidence in support of the theory that regional metamorphism is due to the dynamical and chemical effects of mechanical movement acting on crystalline and elastic rocks. It is also clear that regional metamorphism need not be confined to any particular geological period, because in the N.W. Highlands, both in Pre-Cambrian time and after the deposition of the Durness Limestone (Lower Silurian), crystalline schists and gneiss were produced on a magnificent scale.

2. "On the Horizontal Movements of Rocks, and the relation of these movements to the formation of Dykes and Faults and to Denudation and the thickening of Strata." By William Barlow, Esq., F.G.S.

The paper commenced with a description of some horizontal movements of rocks caused by gravitation; and the author quoted Mr. C. E. Dutton's descriptions of the Grand Cañon District, especially noting the fact that between succeeding escarpments the strata dip slightly from the crest of the one below to the foot of the next above, and that whilst the strata of the median parts of each terrace are nearly horizontal, the inclination increases as we approach the escarpment of the next higher terrace, and also that Dutton observed indications



of a slight elevation of the unloaded strata within the denuded elliptical area known as the "San Rafael Swell." After alluding to Dutton's suggestion that the phenomenon referred to is analogous to the action of creeping in deep mines, the author discussed the nature of such "creeps," which he defined as the thickening of the parts of beds from which a load of superincumbent rock has been lifted, caused by a thinning of the adjoining parts which remained loaded, some of the substance of the latter having been squeezed out to furnish the material for the thickening, and suggested that some of the subsidiary plications found on the flanks of mountains are caused by the thrusts arising from creeps. He also paralleled the fissures in the precipices of the Grand Cañon District with those produced in the pillars of coal owing to the strain induced by the slight inequality in the yielding of the bed supporting it, and pointed out how such fissures would facilitate denudation, giving instances recorded by Dutton, and that an appreciable influence might be thus produced in all cases of mountain-denudation.

The author next considered the case of a body of molten rock below a considerable mass of solid rock. The pressure upon the molten mass would cause movement to take place towards the point where the superincumbent weight was least, provided that absolute equilibrium did not exist. The overlying rocks being more or less plastic, some horizontal movement of the solid rocks at the confines of the molten mass, and subjected to its influence, might be looked for. Any such yielding would tend to draw apart the solid crust resting upon the molten rock, and the ground would open along lines of weakness, such as would be produced by the presence of joints, the crust in some cases breaking up into larger or smaller fragments.

When a large mass of molten matter occurred near the surface, and a fissure was produced in the way described, the weight of the ruptured crust would, if the plastic mass beneath were sufficiently liquid, cause the latter to rise in the fissure, producing dykes. Attention was called to the fact observed by Dutton that basaltic vents frequently occur on the brink of cliffs, but never at their bases; also to the existence of dykes having a strike parallel to the Colorado River. In most cases the vertical fissures which received the molten rock would begin to open from below, and the upper strata might altogether escape rupture.

The author discussed the case of the Henry Mountains, and explained the formation of flat-topped and flat-bottomed dykes according to his views. He next called attention to the influence which the motions of the rocks had exercised in determining directions of drainage when fissures left unfilled became occupied by streams. He next alluded to river-valleys, the existence of which had been accounted for by "antecedent" and "superimposed" drainage, and suggested difficulties in the way of accepting the explanations hitherto advanced, and considered them to be instances of fissuring produced by movements of the strata due to the pressure of a mass of molten or highly plastic rock spreading laterally.

After treating of the formation of faults with normal hade, which



he referred in some cases to rupture of the solid crust by the spread of a vast mass of viscous matter lying beneath it (the faults being sometimes replaced above by monoclinical folds), he referred in conclusion to the extent of the horizontal compression of the earth's superficial crust which is seen to be associated with the elevation of mountain-ranges, and called attention to some evidence that the thickening of the strata caused thereby would be more considerable and general than ordinarily supposed.

3. "Notes on a Recent Discovery of *Stigmaria ficoides* at Clayton, Yorkshire." By Samuel A. Adamson, Esq., F.G.S.

May 9.—W. T. Blanford, LL.D., F.R.S., President, in the Chair.

The following communications were read:—

1. "The Stockdale Shales." By J. E. Marr, Esq., M.A., Sec.G.S., and Prof. H. A. Nicholson, M.D., D.Sc., F.G.S.

The Stockdale Shales extend in an E.N.E.—W.S.W. direction across the main part of the Lake District, parallel with the underlying Coniston Limestone Series and the overlying Coniston Flags, with both of which they are conformable. They also occur in the neighbourhood of Appleby, and in the Sedbergh district. They are divisible into a lower group of black and dark grey and blue Graptolite-bearing shales, interstratified with hard bluish-grey mudstones containing Trilobites and other organisms, and an upper group of pale greenish-grey shales, with thin bands of dark Graptolitic shales. The lower group (Skelgill Beds) are well seen in the stream which runs past Skelgill Farm, and enters Windermere near Low Wood; while the upper group (Browgill Beds) occurs fully developed in the Long Sleddale Valley, and its beds are very fossiliferous in Browgill.

The authors divide these shales into a series of fossil-zones in the following order:—

Stockdale Shales	Browgill Beds	Upper	Bb 2	
			Bb 1	
		Lower	Ba 2 zone of <i>Monograptus crispus</i> .	
			Ba 1 ———— <i>turriculatus</i> .	
	Skelgill Beds	Upper	Ac 5 ———— <i>Rastrites maximus</i> .	
			Ac 4 ———— <i>Acidaspis crinaceus</i> .	
			Ac 3 ———— <i>Monograptus spinigerus</i> .	
			Ac 2 ———— <i>Ampyx aloniensis</i> .	
			Ac 1 <i>Monograptus Clingani</i> band.	
		Middle	Ab 6 Barren band.	
			Ab 5 zone of <i>Monograptus convolutus</i> .	
			Ab 4 ———— <i>Phacops glaber</i> .	
			Ab 3 ———— <i>Monograptus argenteus</i> .	
			Ab 2 ———— <i>Enerinurus punctatus</i> .	
			Ab 1 ———— <i>Monograptus finbriatus</i> .	
		Lower	Aa 2 ———— <i>Dimorphograptus confertus</i> .	
			Aa 1 ———— <i>Diplograptus acuminatus</i> & <i>Atrypa flexuosa</i> .	

Of these zones, the lowest varies, occurring as a thin limestone in Skelgill, with *Atrypa flexuosa*, n. sp., and as Graptolitic shale at Browgill with *Diplograptus acuminatus*, Nich. The others appear

to run persistently across the district, with the exception of the zone of *Rastrites maximus*, which has only been discovered in the Sedbergh area. The thicknesses, lithological characters, and fossil contents of these zones were considered, and comparisons made between these beds and the corresponding deposits of other areas. The whole group attains a thickness of from 250 to 400 feet, of which the Skelgill beds usually make up about one quarter.

The authors correlate the Graptolite-zones with those of the Birkhill and Gala groups of Professor Lapworth as follows:—

Lake District.		South of Scotland.
Zone of <i>Monograptus crispus</i> .....	=	Zone of <i>M. exiguus</i> .
„ — <i>turriculatus</i> .....		Not separated.
„ <i>Rastrites maximus</i> .....	=	Zone of <i>R. maximus</i> .
„ <i>Monograptus spinigerus</i> .....	}	„ <i>Monograptus spinigerus</i> .
<i>Monograptus Chigani</i> band .....		
Not represented? .....		„ <i>Petalograptus cometa</i> .
Zone of <i>Monograptus convolutus</i> .....	}	Zone of <i>M. gregarius</i> .
„ — <i>argenteus</i> .....		
„ — <i>fimbriatus</i> .....		
„ <i>Dimorphograptus confertus</i> .....	=	„ <i>Diplograptus vesiculosus</i> .
„ <i>Diplograptus acuminatus</i> .....	=	„ <i>D. acuminatus</i> .

The zones of *M. convolutus*, *M. argenteus*, and *M. fimbriatus* contain abundance of *M. gregarius*, and the zone of *Dimorphograptus confertus* also contains *Diplograptus vesiculosus* in considerable numbers.

The beds were also compared with the corresponding beds in Sweden, Bohemia, Bavaria, &c., and the fossils other than Graptolites were shown to occur elsewhere in strata of Llandovery-Tarannon age, from which it was concluded that the Stockdale Shales occupy that horizon.

A fault occurs everywhere between the Middle and Lower Skelgill Beds, except perhaps in the Sedbergh district; but it does not seem to cut out a great thickness of rock, and the authors gave reasons for supposing that it was produced by one set of beds sliding over the other along a plane of stratification.

The beds are found to thicken out in an easterly direction, and the possibility of the existence of land in that direction was suggested.

The authors directed attention to the importance of the Graptolitoidea as a means of advancing the comparative study of the stratified deposits of Lower Palæozoic age.

A description was given of the following new species and varieties:—*Placops elegans*, Boeck & Sars, var. *glaber*, *Cheirurus bimucronatus*, Murch., var. *acanthodes*, *Cheirurus moroides*, *Acidaspis crinaceus*, *Harpes judæ*, *H. angustus*, *Ampyx aloniensis*, *Proctus brachypygus*, and *Atrypa flexuosa*.

2. “On the Eruptive Rocks in the Neighbourhood of Sarn, Caernarvonshire.” By Alfred Harker, Esq., M.A., F.G.S.

The rocks in question occupy an area about 5½ miles long from north to south and 2½ miles broad near the south-western extremity of Caernarvonshire. They were described by the author under the following heads:—

(i.) Granite occupying the northern and north-western part of the district. The ordinary type is a fairly normal biotite-granite; but a variety at Meillionydd shows an exceptional structure, the biotite moulding the other constituents in ophitic fashion. The granites are intrusive in the Arenig shales.

(ii.) Gabbro, diorite, &c. in two small patches only. The rock, originally a gabbro, passes into diorite, the diallage becoming amphibolized, and the iron-ores disappearing with the production of granular sphene. Near a bounding-fault this hornblende rock becomes locally schistose and gneissic.

(iii.) Diabase, in the centre, forming the mass of Mynydd-y-Rhiw, and occurring in dykes and sheets near Sarn.

(iv.) Hornblende-diabase showing various relations between the augite and hornblende. Besides the conversion of the former mineral to the latter, a closely similar hornblende has grown as an original border to augite-nuclei. The "secondary enlargement" of hornblende-crystals is also exhibited.

(v.) Hornblende-picrite in several varieties, forming stratiform banks to a thickness of 250 feet and surmounted by hornblende-diabase. The two rocks seem to be in close relation to one another, and to have been injected as laccolites between the Upper Arenig strata near Penarfynydd and Rhiw.

(vi.) Dolerite-dykes cutting all the other rocks, and probably Post-Carboniferous and Pre-Permian.

With the exception of the last all these rocks were referred, on such evidence as is available, to the Bala age.

## LXVI. *Intelligence and Miscellaneous Articles.*

### MOUNTAIN FORMATION.

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

I AM glad that Prof. Le Conte has called attention to the views of American geologists regarding the boundaries of the Appalachian Chain. When I suggested\* that the Appalachians were only *apparently* an exception to the general rule, that a great mountain-range consists of a central gneissic core with largely involved rocks on either flank, I was aware that many American geologists consider that the folded sedimentaries making up what Prof. Le Conte calls the "Appalachians proper," were originally laid down against continental land, of which the crystalline rocks to the eastward are the remnants.

Though much indebted to the ideas of American geologists, I confess that this is one I have never been able to assimilate.

In what respects do these crystalline rocks differ from those of the Alps and other mountain-ranges in which they form the core? They are both folded with the sedimentaries, and although denu-

\* "The Geological Consequences of the Discovery of a Level-of-no-Strain in a Cooling Globe" (Phil. Mag. March).

dation has gone much further in the Appalachians than in the Alps, Palæozoic rocks are still found in troughs of the gneiss. The Silurians that are in contact with the gneiss are, according to the Sections by H. D. Rogers, the most folded of the sedimentaries, and as we recede to the westward the sedimentaries take on more gentle folds.

Considering that the sedimentaries are estimated to be from 8 to 10 miles thick, and that in mountain-chains the site of the thickest deposits is also usually the site of greatest disturbance, it is difficult to realize how the sedimentaries at the thinning-off on a shore-line could be the most folded.

When also, on the other hand, we see, as in the Urals—which in respect of age may be more correctly compared with the Appalachians,—the same folded crystalline rocks forming the core, we should have very good grounds indeed before we make an exception of the Appalachians. A comparison of the little maps of the Appalachians and of Switzerland in my 'Origin of Mountain-Ranges' will, I think, show that if one side of the gneiss core of the Alps were denuded, as I imagine has been the case with the Appalachians, both maps would present very much the same appearance.

I offer the idea only as a suggestion. That the sedimentaries should be unconformable to the Archæans is what might be expected; and, I venture to think, is no proof that the gneissic area of the Appalachians has not been deeply covered with sedimentaries since denuded.

At the same time I freely admit that either view may prove tenable, and shall only be too glad to get further light upon the subject.

Yours &c.,

May 25th, 1888.

T. MELLARD READE.

#### ON THE MAGNETISM OF ORGANIC BODIES. BY S. HENRICHSEN.

The following summary of the results of a series of researches on organic compounds belonging to the fatty-acid series is given by the author:—

1. All bodies as yet examined are diamagnetic.
2. For each  $\text{CH}_2$  which is introduced into the formula of a body, the molecular magnetism increases by a value which in the mean is 163.2, taking the molecular magnetism of water at 10.
3. The molecular magnetisms of isomeric and metamerie bodies are equal if the manner of binding is the same.
4. The molecular magnetism, however, depends on the mode of binding of the atoms. A double binding appears to diminish the molecular magnetism.
5. The specific magnetism for primary and normal compounds is greater than for secondary and iso- compounds; in like manner it is greater for the acids than for the corresponding ethers.
6. With certain assumptions the atomic magnetisms of the in-



dividual elements may be calculated from the present observations. The values I found are :—

H.	O'.	O''.	C'.	C''.	Cl <sub>1</sub> '.	Cl <sub>1</sub> ''.	Cl <sub>1</sub> '''.
9.0	129.0	17.0	145.2	98	282	249	218
Cl <sub>1</sub> iv.	Br <sub>1</sub> '.	Br <sub>1</sub> ''.	Br <sub>1</sub> '''.	I <sub>1</sub> '.	I <sub>1</sub> ''.	S <sub>1</sub> '.	
194	413	374	334	642	577	284	

7. The atomic magnetisms of the halogens decrease as the number of atoms increases.—Wiedemann's *Annalen*, No 5, 1888.

ON THE INSULATING ACTION OF VARIOUS SUBSTANCES IN  
RESPECT OF RADIANT HEAT. BY H. C. VOGEL.

The heat from a Locatelli's lamp fell upon a sensitive thermopile, which was within a metal box with double sides filled with water, and only opened towards the lamp. In front of this the materials to be investigated were placed in the form of plates. The transmissibility of the various substances was measured by the increase of temperature which the front solderings experienced when a stationary condition had been established.

The screening action of the polished metals was greatest ; the thickness of the metal layer was almost entirely without influence. Glass and ebonite kept heat off the least. The most perfect protection was afforded by several polished plates separated from each other by layers of air. The following table gives the increase of temperature over that of the surrounding air which was attained with the various materials :—

Glass	(3.3 mm. thick)	.....	10.9
Ebonite	(5.0 " " )	.....	8.7
Pine wood	(5.9 " " )	.....	6.6
Felt	(7.5 " " )	.....	4.9
Brass	(0.2 to 0.4 mm. thick)	.....	1.0
Tin foil	(0.02 mm. thick)	.....	0.7
Lead foil	(0.08 " " )	.....	0.7
Double screen of tin-plate with an air-space.			0.2

Direct radiation gave in the galvanometer 30°. Hence, if on a system (for instance pillars which support instruments) slow changes of temperature are to be transmitted as uniformly as possible, they must be protected by bad conductors ; rapid local changes of temperature, for instance, such as are caused by the bodily heat of the observer or by the lamps) are best kept off by polished metal screens, behind which the air can circulate freely.

As regards protection against changes in the external temperature, similar rules hold generally, except that in this case the thickness of the protecting layer comes into play.—*Astron. Nachricht.* vol. cxviii. p. 97.—*Beiblätter der Physik*, No. 4, 1888.

## INDEX to VOL. XXV.

- ABERCROMBY** (Hon. R.) on the height, length, and velocity of ocean waves, 263.
- Affinity**, remarks on residual, 297.
- Agate pebbles** from the Soudan, on some, 442.
- Air**, on the determination of the amount of moisture and carbonic acid in the, 77.
- Appalachians**, on the formation of the, 450, 521.
- Apparatus**, new, for measuring radiation, 76; for illustrating the fall of bodies in a vacuum, 77.
- Armstrong** (H. E.) on valency, 21.
- Arrhenius** (S.) on the action of light upon the electrical conductivity of the haloid salts of silver, 451.
- Aubel** (E. v.) on the influence of magnetism and temperature on the electrical resistance of bismuth and its alloys with lead and tin, 191.
- Avogadro's law**, on, 81.
- Ayrton** (Prof. W. E.) on the efficiency of incandescent lamps with direct and alternating currents, 476; on the magnetic circuit of dynamo machines, 496.
- Ball** (Prof. V.) on eroded agate pebbles from the Soudan, 442; on the mode of transport of fragments of granite and other rocks imbedded in the Carboniferous Limestone near Dublin, 443.
- Barlow** (W.) on the horizontal movements of rocks, 517.
- Bell** (L.) on the absolute wavelength of light, 245, 350.
- Bellati** (M.) on the influence of light on the thermal conductivity of crystallized selenium, 324.
- Bismuth**, on the influence of magnetism and temperature on the electrical resistance of, and its alloys with lead and tin, 191; on the effect of magnetization on the thermoelectrical properties of, 285.
- Blake** (Prof. J. F.) on the Cambrian of North-west Caernarvonshire, 239; on the Monian system, 439.
- Blakesley** (T. H.) on the conditions of maximum efficiency in the transmission of power by alternating electric currents, 30; on a method of determining the difference in phase of two harmonic currents of electricity having the same period, 295.
- Boltzmann** (Prof. L.) on some questions in the kinetic theory of gases, 81.
- Bonney** (Prof. T. G.) on stratified palæozoic rocks near Morlaix, Brittany, 71; on the Obermittweida conglomerate, 72; on the Huronian series of Sudbury (Canada), *ib.*
- Books**, new:—Unwin's Testing of Materials of Construction, 436; Transactions of the Edinburgh Geological Society, 514.
- Bosanquet** (R. H. M.) on the use of the term "resistance," 419.
- Boys** (C. V.) on experiments with soap-bubbles, 409.
- Braun** (F.) on the emission of light by ignited bodies, 242.
- Buchanan** (J.) on a law of distribution of molecular velocities amongst the molecules of a fluid, 165.
- Burbury** (S. H.) on the diffusion of gases, 129.
- Capillary analysis**, on, 244.
- Carbon**, mathematical spectral analysis of, 343.
- Carnot's theorem**, on an extension of, 512.
- Cauchy's theory** of double refraction, on, 116.

- Cerium in the sun, on, 3.
- Cole (G. A. J.) on some occurrences of Tachylite, 321.
- Copper, on the application of the electrolysis of, to the measurement of electric currents, 179.
- Cryohydrates, on the thermodynamics of, 406.
- Dale (T. P.) on the numerical relation between the index of refraction and the wave-length within a refractive medium, and on the limit of refraction, 326.
- Davison (C.) on the movement of scree-material, 320.
- Dedekind's theorem, on, 40.
- Deeley (R. M.) on a theorem of glacier motion, 156.
- De Rance (C. E.) on the Cae-Gwynn cave, 447.
- Diffusion, application of the, of gases and vapours through porous bodies to determine the amount of moisture and carbonic acid in the surrounding air, 77; of gases, on the, 129.
- Draper (Dr. C. H.) on the polarization of platinum plates, 487.
- Dynamo machines, on the magnetic circuit of, 496.
- Earth, on the results of the contraction through cooling of a solid, 7, 210.
- Ebert (H.) on the influence of light upon the electric discharge, 162.
- Edgeworth (F. Y.) on a new method of reducing observations relating to several quantities, 184.
- Electric currents, on the conditions of maximum efficiency in the transmission of power by alternating, 30; on the application of the electrolysis of copper to the measurement of, 179.
- discharge, on the influence of light upon the, 162.
- endosmose and allied phenomena, on, 52.
- Electrical impressions, on, 80.
- phenomena provoked by radiation, on some, 314.
- transportation in liquids, 78.
- Electricity, on, produced by the friction of drops, 75; on the velocity of, 153; on the convection of, by evaporation, 244; on a method of determining the difference in phase of two harmonic currents of, having the same period, 295.
- Electrolysis, experiments on, 276.
- Electromagnetic waves, on, 130, 202, 379.
- Electromotive force of voltaic cells, application of the deci-ampere or centi-ampere balance to determine the, 164.
- — in moving conductors, on the, 271.
- Elster (J.) on electricity produced by the friction of drops, 75.
- Endosmose, on the theory of electric, 52.
- Energy, on the distribution of, in the spectra of solids, 425.
- Everett (Prof. J. D.) on the general laws of brightness of images, 216.
- Fisher (Rev. O.) on the results of the contraction of a solid globe, 7.
- Fox (H.) on the gneissic rocks off the Lizard, 438.
- Fresnel's kinematics of double refraction, on, 116.
- Gardner (J. S.) on the Barton and Upper Bagshot formations, 444.
- Gas, on the motion of a, "in mass," 38.
- Gases, on some questions in the kinetic theory of, 81, 172; on the diffusion of, 129.
- Gee (W. W. H.) on the change of density of the electrolyte at the electrodes, 276.
- Geikie (Dr. A.) on the age of the altered limestone of Strath, Skye, 73.
- Geitel (H.) on electricity produced by the friction of drops, 75.
- Geological Society, proceedings of the, 71, 238, 316, 438, 515.
- Geological Survey of the Highlands of Scotland, on the recent work of the, 515.
- Glacier motion, on a theory of, 156, 452.
- Gold, on recent discoveries of, in the Transvaal, 73.
- Goppelsroder (F.) on capillary analysis, 244.
- Gray (T.) on the application of the electrolysis of copper to the measurement of electric currents, 179.
- Green (Prof. A. H.) on the geology of the Cape Colony, 316.

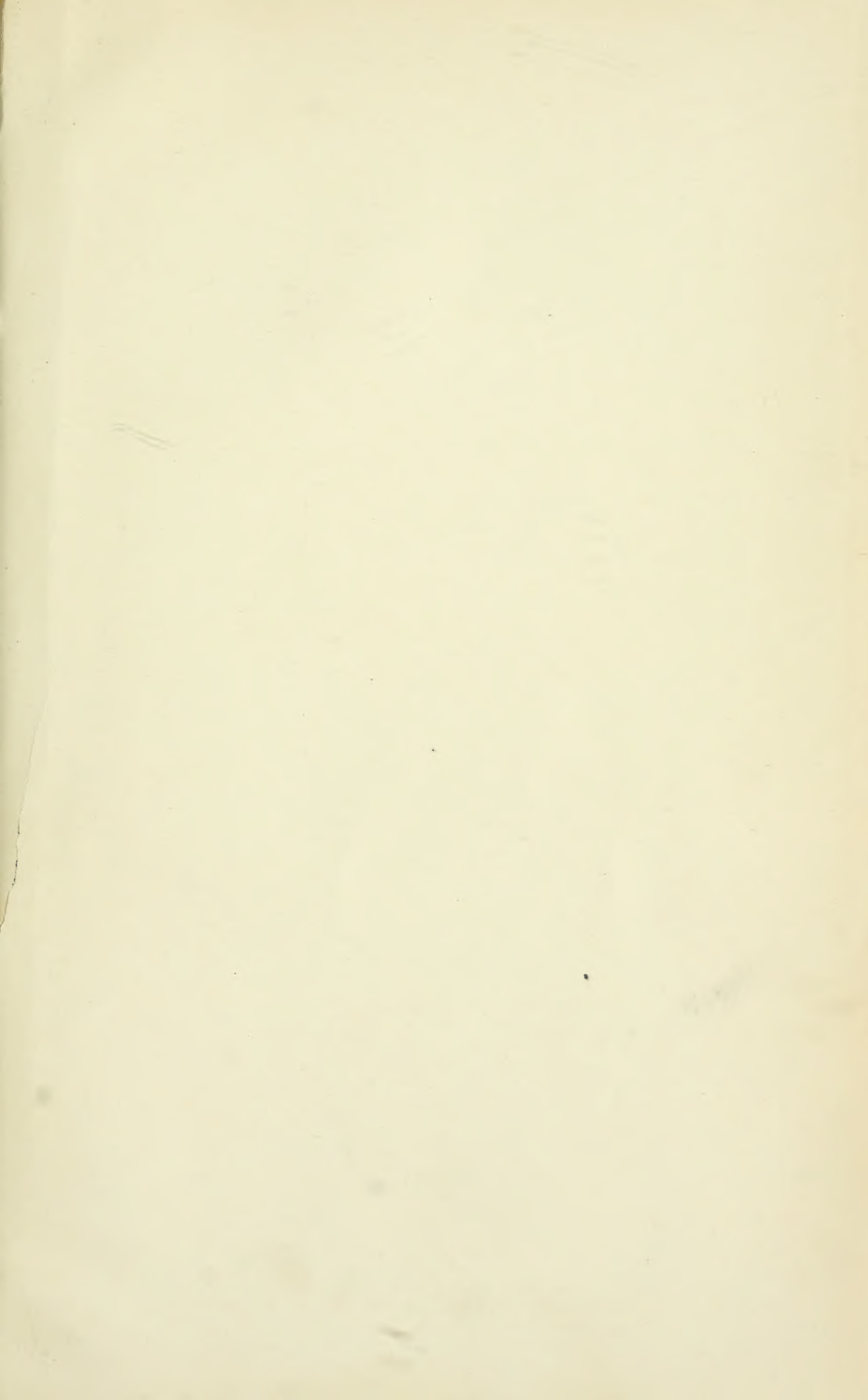
- Green's theory of double refraction, on, 116.
- Grünwald (Dr. A.) on the mathematical spectral analysis of magnesium and carbon, 343.
- Harker (A.) on eruptive rocks in S.W. Caernarvonshire, 520.
- Heat, on the insulating action of various substances in respect of radiant, 523.
- Heaviside (O.) on electromagnetic waves, and the forced vibrations of electromagnetic systems, 130, 202, 379.
- Helmholtz's definition of valency, on, 21.
- Henrichsen (S.) on the magnetism of organic bodies, 522.
- Heyes (J. F.) on the tetravalency of oxygen, and on the varying valency of the elements, 221; on valency, validity, and residual affinity, 297.
- Hicks (Dr. H.) on the Cae-Gwynn cave, 446.
- Hill (W.) on the lower beds of the Upper Cretaceous series in Lincolnshire and Yorkshire, 445.
- Holden (H.) on the change of density of the electrolyte at the electrodes, 276.
- Hughes (Prof. T. M'K.) on the position of the Obermittweida conglomerate, 71.
- Hunt (Dr. T. S.) on the integral weight of water, 269.
- Hutchins (Prof. C. C.) on a new instrument for the measurement of radiation, 76.
- Ilygrometer, new, 78.
- Images, on the general laws of brightness of, 216.
- Induction, on the variation of the coefficients of, 453.
- Influence-machine, on a modified water-dropping, 283.
- Iron, on the effect of temperature on the thermoelectrical properties of, 45; on the recalc-scence of, 103.
- Irving (Rev. A.) on the Bagshot beds of the London basin, 241; on the Red-Rock series of the Devon coast section, 241.
- Keeping (H.) on the Barton and Upper Bagshot formations, 444.
- Kohlrausch (F.) on the thermal conductivity of hard and soft steel, 448.
- Lamb (Prof. H.) on electric enlos-  
mose, and on the existence of a sliding coefficient for a fluid in contact with a solid, 52.
- Lamps, on the efficiency of incandescent, with direct and alternating currents, 476.
- Lecher (Dr. E.) on the convection of electricity by evaporation, 241.
- LeConte (Prof. J.) on mountain formation, 450; on glacial motion, 452.
- Light, on the influence of, upon the electric discharge, 162; on the emission of, by ignited bodies, 242; on the absolute wave-length of, 245, 343; on the influence of, on the thermal conductivity of crystallized selenium, 324; on the action of, on the electrical conductivity of the haloid salts of silver, 457.
- Lightning-rods, on the price of the factor of safety in the materials for, 170.
- Liquid ellipsoid, on the motion of a, under its own attraction, 40.
- Liquids, on electrical transportation in, 78.
- Love (A. E. H.) on the motion of a liquid ellipsoid under its own attraction, 40.
- Love (E. F. J.) on discriminating real from accidental coincidences between the lines of different spectra, 1.
- Lusana (E.) on the influence of light on the thermal conductivity of crystallized selenium, 324.
- Magnesium, mathematical spectral analysis of, 343.
- Magnetic circuit, on the, of dynamo machines, 496.
- fields, on the production of intense, 322.
- Magnetism, on the influence of, on the electrical resistance of bismuth and its alloys with lead and tin, 191; on the, of organic bodies, 522.
- Magnetization, on the effect of, on the thermoelectrical properties of bismuth, 285.
- Marr (J. E.) on the Stockdale shales, 519.
- Maxwell's law of the distribution of velocities, on the proof of, 89.
- Michelson (W.) on the distribution of energy in the spectra of solids, 425.

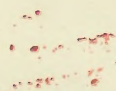


- Mountain formation, on, 450, 521.
- Newall (H. F.) on the recalcence of steel, 510.
- Nicholson (Prof. H. A.) on the Stockdale shales, 519.
- Nickel, on the temperature at which, begins to lose its magnetic properties, 372.
- Obermayer (A. V.), experiments on St. Elmo's fire, 323.
- Oldham (R. D.) on the law governing the action of flowing streams, 240.
- Organic bodies, on the magnetism of, 522.
- Oxygen, on the tetravalency of, 221.
- Parker (J.) on the thermodynamics of cryohydrates, 406; on an extension of Carnot's theorem, 512.
- Perry (Prof. J.) on the efficiency of incandescent lamps with direct and alternating currents, 476; on the magnetic circuit of dynamo machines, 496.
- Polarization of platinum plates, on the, 487.
- Post-glacial time, an estimate of, 319.
- Power, on the conditions of maximum efficiency in the transmission of, by alternating electric currents, 30.
- Prestwich (Prof. J.) on the correlation of some of the Eocene strata in the Tertiary basins of England, Belgium, and the North of France, 238.
- Puluj (J.) on an apparatus for illustrating the fall of bodies in a vacuum, 77.
- Pumps, on the application of hydraulic power to mercurial, 313.
- Quantities, on a new method of reducing observations relating to several, 184.
- Radiation, on a new instrument for the measurement of, 76; on some electrical phenomena provoked by, 314.
- Reade (T. M.) on the consequences of a level-of-no-strain in a cooling globe, 210; on an estimate of post-glacial time, 319; on tidal action as an agent of geological change, 338; on mountain formation, 521.
- Recalcence, experiments on, 110; of steel, 510.
- Refraction, on Fresnel's kinematics of double, 116; on the numerical relation between the index of, and the wave-length within a refractive medium, and on the limit of, 326.
- "Resistance," on the use of the term, 419.
- Righi (Prof. A.) on some electrical phenomena provoked by radiation, 314.
- Rimington (E. C.) on the measurement of the power supplied to the primary coil of a transformer, 482.
- Rocks, on stratified Palæozoic, near Morlaix, Brittany, 71; on the horizontal movements of, 517; on eruptive, in S.W. Caernarvonshire, 520.
- St. Elmo's fire, experiments on, 323.
- Schidlowsky (F.), application of the diffusion of gases and vapours through porous bodies to determine the amount of moisture and carbonic acid in the surrounding air, 77.
- Selenium, on the influence of light on the thermal conductivity of crystallized, 324.
- Silver, on the action of light on the electrical conductivity of the haloid salts of, 451.
- Sliding coefficient, on the existence of a, for a fluid in contact with a solid, 52.
- Smith (F. J.) on the application of hydraulic power to mercurial pumps, 313.
- Soap-bubbles, experiments with, 409.
- Spectra, on discriminating real from accidental coincidences between the lines of different, 1; on the distribution of energy in the, of solids, 425.
- Spectral analysis, mathematical, of magnesium and carbon, 343.
- Steel, on the thermal conductivity of hard and soft, 448; on the recalcence of, 510.
- Stefan (Prof.) on the production of intense magnetic fields, 322.
- Stratigraphical table for Western Europe, on a, 274.
- Streams, on the law governing the action of flowing, 240.
- Sumpner (W. E.) on the variation of the coefficients of induction, 453.
- Surface-elevations, on the mean height of, 7.

- Tachylite, on some occurrences of, 321.
- Tait (Prof. P. G.) on the motion of a gas "in mass," 38; on some questions in the kinetic theory of gases, 172.
- Tereschin (S.) on electrical transportation in liquids, 78.
- Thermoelectrical properties of iron when under stress or strain, on the, 45; of bismuth, on the effect of magnetization on the, 285.
- Thompson (Prof. S. P.) on the price of the factor of safety in the materials for lightning-rods, 170; on a modified water-dropping influence-machine, 283.
- Thomson (Sir W.) on Cauchy's and Green's doctrine of extraneous force to explain dynamically Fresnel's kinematics of double refraction, 116; on Kirchhoff's investigation of the velocity of electricity, 155; on the application of the deci-ampere or centi-ampere balance to determine the electromotive forces of voltaic cells, 164.
- Tidal action as an agent of geological change, on, 338.
- Tomlinson (H.) on the effect of temperature on the thermoelectrical properties of iron, 45; on the recalcence of iron, 103; on the effect of magnetization on the thermoelectrical properties of bismuth, 285; on the temperature at which nickel begins to lose its magnetic properties, 372.
- Tschechowitsch (K.) on electrical impressions, 80.
- Valency, remarks on, 21, 297; remarks on the varying, of the elements, 221.
- Velocities, on a law of distribution of molecular, amongst the molecules of a fluid, 165.
- Vibrations, on the forced, of electromagnetic systems, 130, 202.
- Vogel (H. C.) on the insulating action of various substances in respect of radiant heat, 523.
- Water, on the integral weight of, 269.
- Watson (Dr. H. W.) on the electromotive force in moving conductors, 271.
- Waves, on electromagnetic, 130, 202, 379; on the height, length, and velocity of ocean, 263.
- Wethered (E.) on insoluble residues from the carboniferous limestone at Clifton, 319.
- Wiedemann (Prof. E.) on the influence of light upon the electric discharge, 162.
- Young (Dr. J.) on a stratigraphical table chiefly for Western Europe, 274.

END OF THE TWENTY-FIFTH VOLUME.







QC

The Philosophical magazine

1

P4

ser.5

v.25

Physical &  
Applied Sci.  
Serials

PLEASE DO NOT REMOVE  
CARDS OR SLIPS FROM THIS POCKET

---

UNIVERSITY OF TORONTO LIBRARY

---

